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THE Psychological Review

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JAMES R. ANGELL, University of Chicago (*Monographs*) and

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THE PSYCHOLOGICAL REVIEW

THE FATHER OF MODERN PSYCHOLOGY

BY PROFESSOR FOSTER WATSON

The 'father' of modern psychology was, I suggest, Juan Luis Vives. It may be objected that if we take 'modern' in a sufficiently literal sense, we must go back to Aristotle. For that great philosopher traced the origin and development of the pre-Socratic psychology, critically examined the Platonic views, embodied and organized from his predecessors whatever would serve as basis; and for the rest, supplied, from his own researches and thought, an organic system of psychology, which has held its own, and still claims close study—from the fourth century B.C. to the present time—continuously for over twenty-two centuries and a half. St. Thomas Aquinas, fifteen hundred years after Aristotle, re-affirmed the main points of Aristotelian psychology, but supplemented them by a rationalized interpretation, in which he, in many cases, anticipated modern psychological thought.[1]

Doubtless the line of continuity in psychological advance is traceable, in this way, to Aristotle as the real founder of the subject. But, our modern division of history, into ancient, mediæval and modern, requires us to consider new starting-points, though, logically, in the light of the principle of continuity, it is misleading to regard even these great divisions as abrupt transitions. Although the Renascence period of the fifteenth and sixteenth centuries brought a steady concentration of attention upon psychological questions, as it did upon all humanistic problems, yet the main

[1] The Rev. Prof. Michael Maher, S. J., has shown in his interesting 'Psychology—Empirical and Rational' the parallels between St. Thomas Aquinas and modern psychology.

setting of psychological theory had clearly been determined by those great thinkers, Aristotle and St. Thomas Aquinas; and any advance was most hopefully to be looked for, which started from them as basis.

It is often stated that 'the father' of modern psychology was René Descartes (1596–1650). Without attempting to withdraw any credit from the actual accomplishments of Descartes, in his day and generation, in the subject of psychology, it would be extraordinary, on *à priori* grounds, if the period of the Renascence (say from 1450 onwards) till the time of the birth of Descartes (1596) had failed to produce a conspicuous thinker on a subject so essentially humanistic, as that of the Mind. Some writers, accordingly, place Francis Bacon (1561–1626) as the pioneer of modern psychology. Bacon was, certainly, the most influential advocate of the empirical scientific method of the seventeenth century. Psychology has made its great advance by the employment of this method. Hence, it is urged, Bacon is the leader to whom modern psychology traces its beginning. But neither Bacon nor Descartes was the first Renascence writer to give his attention to psychological theory, nor even to the advocacy of the empirical inductive method. In a wide though valid sense of the term, every man is a psychologist, and every man employs the inductive method, and the 'fatherhood' of both goes back to an antiquity, not merely as old as Aristotle, but as old as man himself, at least. But the *self-conscious emphasis on induction* as a method of inquiry and discovery in philosophical, and particularly in psychological questions, must be taken back, even in Renascence times, beyond and before Descartes and Bacon, at any rate, to Juan Luis Vives (1492–1540).

Thus, Vives shows explicitly his insight into the significance of the empirical inductive method in his account of the origin of the arts. For the formulation of the arts, says Vives, was due to observation joined with reasoning. "In the beginning, first one, then another experience, through wonder at its novelty, was noted down for use in life; from a number of separate experiments the mind gathered a universal law,

which, after support and confirmation by many experiments was considered certain and established. Then this knowledge was handed down to posterity. Others added subject-matter which tended to the same use and end. This collection of knowledge-material by men of great and distinguished intellect, constituted the several branches of knowledge, or the arts. . . . Whatever is in the arts was in nature first, just as pearls are in shells, or gems in the sand."[1] The insight which Vives brought to bear on the place of the empirical, inductive method, in the building up of the arts and sciences, was of the first importance to him, when he came to study the subject of psychology, theoretically.

In 1538, Vives published his book on psychology, entitled, after the work of Aristotle: 'De Anima et Vita.' He addressed it to Francis, duke of Béjar, to whose descendant, another duke of Béjar, Cervantes in 1605 dedicated 'El Ingenioso Hidalgo Don Quijote de la Mancha.'

In this Preface to the 'De Anima,' Vives makes clear that he writes on the mind, in no perfunctory or merely conventional spirit, for he recognizes that the knowledge of the mind has relation to matters 'of the *greatest usefulness*.' To govern oneself one must know onself, not only as so much bones and flesh and nerves and blood (though these come into the calculation) but each should learn to observe the nature, quality, ability, strength, passions, of the mind. Each should 'explore' himself in his varied nooks, and even obscurities of mental life. "On this account, it seemed good to me to ponder deeply on some points of this great subject, all the more because recent philosophers have brought but little industry to the study, *content with what had been left behind by the ancients*. They have added nothing at all except problems, almost impossible of solution, and such that, even if solved, would bring no fruitfulness. Formerly the ancients involved themselves in great absurdities on this subject, for they thought wrongly of the mind, which is not perceived by

[1] For an account of the relation between Francis Bacon and Juan Luis Vives see Rudolf Günther, 'Inwieweit hat Ludwig Vives die Ideen Bacos von Verulam vorbereitet' (1912), and as to the influence of Vives on Descartes see, Roman Pade, 'Die Affectenlehre des Johannes Ludovicus Vives' (1893).

the bodily senses—and even their opinions on the very matters which we do perceive by the senses were most inept."

Vives deliberately passes by the method then,—as ever—so much in vogue of rebutting opinions deemed to be false, which he says are more numerous in this than in any other subject of inquiry. Such refutations would lead to thorns rather than fruit.[1] He will attempt to fit into the structure of his discourse, 'those expressions which have sprung from the people and are in common acceptance with them, and then have become unintelligible as they have passed into the use of the learned.' For in dealing with the recondite problems of the mind, the adaptation of a fitting vocabulary is a serious part of the undertaking if a treatise is to be accommodated to learners. Vives's concern for his choice of language and vocabulary, it will be remembered, was reflected, with such good consequences, in his successors Francis Bacon, Thomas Hobbes, and René Descartes, and very specially in John Locke's 'Essay on the Human Understanding,' Book III., than which it would be difficult to find a more masterly exposition of language as the means of expressing ideas clearly.

In his address to the duke of Béjar, then, Vives claims that he has parted company with the mischievous babblings of the Stoics and their carping sophisms, and no less with the hidden and subtle opinions of Aristotle.

In describing the contents of Vives's 'De Anima' it will be more profitable to emphasize his independent and original views rather than to dwell upon the undoubtedly many points of similarity which could be found in his book with the subject-matter in the works of Aristotle and St. Thomas Aquinas.[2]

Tired of the atmosphere of dialectical displays which inflated the pride of victory rather than stimulated the search for truth, Vives was ready to try new paths. The time-honored treatment of psychology included a discussion of the

[1] Vives, however, to some extent, deals with the 'false opinions' of his predecessors in psychology, in his 'De Causis Corruptarum Artium' and in his 'De Veritate Fidei Christianæ.'

[2] See T. G. A. Kater, 'J. L. Vives und seine Stellung zu Aristoteles;' G. Hoppe, 'Die Psychologie des J. L. Vives;' R. Pade, 'Die Affectenlehre des J. L. Vives.'

question What is the soul? Yet Vives ventures to write a book entitled 'De Anima' in which he says: "What the soul *is*, is of no concern for us to know; what it is like, what its manifestations are, is of very great importance."[1] This calm renunciation of the metaphysical aspect in favor of the descriptive account of the activities of the mind is a natural enough result from Vives's revolt from the older academic disputational methods, which precluded progress, because they could not reach further in their conclusions than they assumed in their premises, and their premises were always ultimately founded on *à priori* abstractions. Thus, disputes on the nature of the soul either began in the unknown, and ended there, or else sought an authoritative basis, which was similarly substantially unknown (because not subjected to enquiry). Vives, therefore, excluded the discussion of what the soul is, in its essence, from his scope. Instead, he asks for careful investigation into the *manifestations* of the soul in all the activities of consciousness. "We cannot rightly declare what the soul is in its essence, and as a bare thing place it, as it were, before the eyes, but we can set it forth, clothed and as if painted in a picture, in its own most apt colors, so that it is seen in *its own actions*. For it has not come under the observation of our senses, but we perceive its works (*opera*) by almost all the senses, internal and external. Surely the goodness of the Lord of Nature to us reveals itself by many proofs, on every side, by thus aiding us, in placing before our eyes and in such abundance, these manifestations of the soul. For there is no sign more clear that things are not to be converted to our use than for them to be remote, rare, difficult to be prepared. Nor did he who bid us know ourselves, refer to the essence, but to the actions of our mind so that they may be ordered for moral life, and by the expulsion of vice, we may follow virtue which will so lead us that we may spend in full wisdom, as immortals, the happiest eternity." It is a central doctrine of Vives that Knowledge is of value, simply when it is 'put to use.' The

[1] Anima quid sit, nihil interest nostra scire, qualis autem, et quae eius opera, permultum. 'Opera,' Vol. III., p. 332.

observational method of studying the manifestations of our minds has a value for application which cannot be gainsaid. If indeed, the processes of cognition were to be regarded as only of intellectual worth, even then the right study of the passions, as they show themselves in ourselves and in the individuals we read of in history, would be of direct ethical significance for the purposes of examples or of warnings. And, accordingly, Vives devotes one of the three books of the 'De Anima' to a critical and constructive study of the passions.

We have seen that Vives places the emphasis in psychological studies upon the observation of the manifestations of the soul, in its outward realized activities. When we thus observe the results of mental activity in the numerous forms of cognition feeling, will, in other persons, or in ourselves, we are at the point of view of empirical psychology. Vives may, therefore, be claimed as a pioneer in the advocacy of this method, prior to Francis Bacon and to René Descartes. It was not a long step to take, to advance from the empirical psychology, which is concerned in tracing the processes of mental activity in others to that of recording, by way of psychological investigation, the results of interrogation as to what has happened in one's own mental experiences. Hence, illustrations can be found in Vives, of the conscious employment of the introspective, empirical method, the method that is especially characteristic of later psychological students, and as these appear to be the first instances of conscious introspective interrogation of consciousness in psychological investigation in modern times, *i. e.*, from the Renascence onwards, they are of more than ordinary interest and significance.

"As often," says Vives, "as I see a house at Brussels, which is opposite to the Royal Palace, Idiaqueus comes into my mind, for he is its occupier. Very often in that house,as far as his business would allow, we used to chat over matters pleasant to both of us. Now, as often as I revolve the idea of Idiaqueus I do not think of the Palace, because the memory of my friend and his house is more noteworthy to me than

the idea of the Royal Palace.[1] So with sounds, tastes, smells. When I was a boy at Valencia, I was ill of a fever. Whilst my taste was deranged, I ate cherries. For many years afterwards, whenever I tasted fruit I not only recalled the fever, but also seemed to experience it again."[2]

These illustrations of the introspective method occur in connection with Vives's treatment of two associated ideas [*recordatio gemina*]. Of course, it is not the fact, as some writers seem to suppose, that the doctrine of association of ideas began with David Hartley or with the Mills nor even with Thomas Hobbes, or John Locke. This explanation of mental process, goes back, at any rate, to Aristotle. But it may be claimed for Vives that his development of Aristotle's theory marks the Renascence advance. No other author as early as Vives contributed so strong and comprehensive an exposition of it, as Vives. Indeed, Sir William Hamilton, the most erudite of all British philosophers, in the history of psychology, said: "Vives's observations comprise in brief, nearly all of principal moment that has been said upon this subject (of mental association) *either before or since.*"[3]

Before Sir William Hamilton, Samuel Taylor Coleridge[4] in his effort to discountenance Hobbes's claim to be the 'original discoverer' of the law of association, as advocated by Sir James Mackintosh hit upon the 'De Anima' of Vives, and appears to have been the first Englishman to have drawn attention to Vives's enunciation of this law of association of ideas. This was in 1817. The words quoted by Coleridge from Vives are: *Quæ simul sunt a phantasia comprehensa si alterutrum occurrat, solet secum alterum representare.*

[1] Vives is explaining that our minds often travel more readily from the less to the greater than *vice-versa*. Some might think that the house oposite to the Palace would lead to the thought of the Palace. But Vives pays a compliment to his friend, as well as expounds psychology, when he says his friend's house recalls the 'greater' *i. e.*, the more excellent idea of his friend and their former talks together.

[2] Vives argues that, for this reason, in mnemonics, the 'clues' to excite memory should not be themselves of such interest as to detain the attention too much from the suggestion of what they are intended to assist in recalling.

[3] 'The Works of Thomas Reid' (including Hamilton's 'Dissertations'), 1872, 7th ed., Vol. II., p. 896, column i.

[4] In the 'Biographia Literaria,' Chapter V.

Coleridge proceeds to regard Vives as "subordinating all other exciting causes of association to *time*. The soul proceeds '*a causa ad effectum, ab hoc ad instrumentum, a parte ad totum*'; thence to the place, from place to person, and from this to whatever preceded or followed, all as being parts of a total impression, each of which may recall the other." Chains of associated ideas may have the most distant links connected 'by the same thought having been a component part of two or more total impressions.' Vives's example quoted by Coleridge is: "From the idea of Scipio I come to the thought of the Turkish power, on account of his victories in that part of Asia in which Antiochus was reigning."

Hamilton, who shows little mercy to Coleridge, tells us that the whole of the latter's chapter on the history of the law of association was 'conveyed,' to use the old expression, from the German Maass, and is a 'blundering plagiarism.' So, with regard to Coleridge's inference that Vives substantially limits the law of association to that of the sequence of connected ideas *in time*, *i. e.*, to the phenomena of recollection, Hamilton clearly shows that Vives does not so limit associative ideas to *time*, or to place, but he points out their operation '*in all the connections of thought and feeling.*'

The insight which Vives thus showed in his exposition of the law of association of ideas prepares us for further details in his experiential account of memory. As we have two hands, so memory is twofold, and consists in apprehending and retaining. The differences in memory amongst men are implanted by nature. Some men like Hortensius remember words more readily; others, *e. g.*, Themistocles. Some apprehend quickly and retain better the curious; others, the simple; some, public affairs, others, private matters; some, the old, others, the new; some, their own affairs, others, those of others; some, vices, others, virtues;—each according to the proneness of his disposition. For he attends more willingly and therefore more closely to this or that, and attention strengthens memory. A natural bodily constitution is of the highest importance to memory, and with such were endowed the great men whose magnitude of memory has been

handed down, *e. g.*, Themistocles, Cyrus, Cineas, Hortensius. This natural endowment is capable of being strengthened by habits of living. Memory is more tenacious in slow than in quick minds, like an impression driven in for a long time on stone or iron, but the swift minds more readily *bring back* a reminiscence. A deep descent into memory is made by those things to which we give close attention and care when first perceived. If anyone is in a state of emotion or excitement on first hearing of something, and this is mingled with the memory, this reminiscence is easier, quicker, longer, as, *e. g.*, for what has entered the mind when in a state of violent grief the memory is the longest, and for that reason, adds Vives, in determining the boundaries of property it is the custom of some races to bring their boys there, and thrash them with severity, so that the boundaries may be more firmly, and the longer, retained in memory. By practice and frequent meditation the memory gathers great strength. Unlike other gifts of the mind, which do not deteriorate by rest and cessation, memory grows fainter and fainter, day by day, if it is not exercised.

The law of forgetfulness is fourfold. (1) When the image painted in the memory is utterly scratched out; (2) when it is smeared or broken in pieces; (3) when it stealthily evades us in our search for its recall; (4) when it is covered over as with a veil, in disease, or in the excitement of emotion. Vives sketches the method of recall of an idea; by retracing as it were, our steps, we may come in thought to what we are seeking. Thus from the idea of a ring we think of a goldsmith, from him to a queen's collar; thence to a war which her husband waged; from the war to the leaders; from the leaders to their ancestors or children; thence to the studies in which the latter are being exercised. To such series of ideas there is no end. These steps spread themselves widely through all kinds of subjects, from cause to effect, and so on, as we have seen in Coleridge's reference. Vives, however, gives an example, which Coleridge omitted, but which we may note, since we are emphasizing the appeal to experience made by Vives. "In thinking of the name of Cicero, there

comes into my memory the name of Lactantius, for he was the imitator of Cicero. From him, I proceed to think of the copper-plate artist, since his book is said to have been either the first or amongst the first printed from copper-plates."

Remembrance is of two kinds. It is either natural, or voluntary, when we pass freely from one idea to another, or it is ordered (jussa) when the mind makes an effort to reach and bring back some idea. Those ideas which occur in a series are more easily remembered, as for instance, in mathematics. Verses are conducive to faithful retention on account of the rhythm of composition, which keeps the mind from straying outside its limits. The art of mnemonics is based on the order of what is committed to memory. It is at this point that Vives introduces his definition of association. For with regard to ideas 'which have been included in the imagination [phantasia] at the same time if one of the two should occur, it is wont to bring back with it, the other.' According to his custom, Vives illustrates this definition. "From the sight of a place there comes into the mind what we know once happened in that place, or what was situated there. When something joyful happened along with a voice or sound, we are delighted when we hear that same voice or sound again. If it was a sad event, we are saddened. This is also to be noted in the lower animals, who if they receive something pleasant, after they have been called by a sound, they will run towards it readily and gladly, on hearing the same call again. But if they were beaten, after being called, they are frightened by the memory of the blows, on hearing the same call again. Returning to memory, another factor in clear memory, Vives points out, is the time-element. A distinction of times is necessary in reminiscence, otherwise images are confused, as if in a picture, other pictures should get painted on the top, after an interval. Those images which we have received with a quiet, leisurely mind imprint their trace for a longer time, and more permanently, if we gave our attention to them. It is for this reason that the things we have seen and heard in early life are recollected by us so clearly. For at that age the mind is free from cares and thoughts. Every-

thing is new. We therefore watched closely these things which won our admiration and they sank deeply into the mind. Older men are preoccupied, and in an 'internal agitation of thoughts' so that they cannot so restfully admit new ideas, or find out readily the old amid their experiences.

In connection with associations by similarity, Vives is impressed by the opportunities for error in memory and in thought, as we pass from like to like. We mistake Georgius for Gregorius; problema for enthymene; Pindarus for Pandarus. Similarity may appear in the beginning, middle, or end, of words. Or the similarity may be in things, from the manner in which we concentrate our attention on them. In philosophy we may think of Xenocrates for Aristotle; in speaking of the Carthaginian Wars we may confuse Scipio with Q. Fabius. In a question of poverty, we may say Irus instead of Codrus. In eloquence, we may ascribe something to Demosthenes instead of to Cicero. In a matter of beauty, we may speak of Narcissus for Adonis. Or in a question of smell, mistakes may be made of garlic for onion. So, in matters of times, actions, qualities. Similarity, therefore, disturbs the memory, as it does the eyes of the body, so that judgment is confused. Vives sees that errors can creep in at the 'first attention' when an idea is received or at the 'second consideration' when reminiscence wrongly draws forth those ideas which were received as wholes. "Yesterday," illustrates Vives, "in the market-place, Peter of Toledo saluted me. I did not notice the fact sufficiently, nor remember it accurately. If anyone asks me: 'Who saluted you yesterday in the market-place?' and adds nothing further, I shall answer more readily than if he were to say: 'Was it John Manricus or Ludovicus Abylensis?' The labor thus becomes two-fold, first to reject what does not fit, secondly to replace it by what is right."

We can thus see that Vives is permeated with the idea of an empirical and introspective method of the modern type in his 'De Anima,' and that the application of this method has taken him far along the path of development of the doctrine of association of ideas, and in the exposition of ob-

servational aspects of memory. There are many other views of Vives which have special interest. Thus his emphasis on the necessity of observing and distinguishing the great variety of differences in men's minds became the basis of the treatise of his fellow countryman, Juan Huarte, written in 1557, and translated into English[1] by Richard Carew in 1594, under the title of the 'Examination of Men's Wits.' This book following on explicit suggestions in Vives's 'De Anima,' demands that children's impulses and tendencies, both in play and at work, should be studied so as to afford a psychological basis for their studies and after-occupations. Vives distinguishes carefully between the *ratio speculativa*, whose 'end' is the truth and the *ratio practica* whose end is the 'good.' The Spanish biographer of Vives, Señor Professor D. Adolfo Bonilla y San Martín, points out the parallel between Vives's view and that afterwards developed by Kant. Vives introduces *à priori* subjective forms of reason which he terms *anticipationes seu informationes naturales*, and as Bonilla remarks, *anticipationes naturales* is also the term used by Francis Bacon in 'Novum Organum,' Book I. Nor will theologians and philosophers, if they turn to Vives's 'De Anima,' pass by without notice his treatment of the problems of free-will, and of immortality, for they present an interesting individual statement of these problems—of this Renascence period.

Educationists ought to realize that Vives writes in the 'De Anima,' a section which he terms 'de Discendi Ratione.' He attempts the well-trodden path of an evaluation of the order of intellectual resources and discipline afforded by the several senses. He remarks: "The course of learning is from the senses to the imagination, and from that to the mind of which it is the life and nature, and so progress is made from singulars to combinations, from singulars to the universal; which is to be noted in boys. . . . And so *the senses are our first teachers*[2] *in whose home the mind is enclosed.*" It is in this section of the 'De Anima' that Vives refers to the ad-

[1] Indirectly, from the Italian of Camillo Camilli.

[2] Cf. Rousseau's often-quoted "Our first teachers of philosophy are our feet, our hands, and our eyes." 'Emile' (Payne's ed.), p. 90.

miration, almost bordering on incredulity, with which he heard that a deaf-mute had become taught, and he points out that the method of learning necessarily implied a large measure of self-teaching.

Whilst the psychological views of Vives, sketched above, are of this most significant modern cast, yet it must be stated clearly that the general setting from which they are taken is that of the old Aristotelian psychology, and if there are modifications in statement, these are usually clearly based on scholastic writers, for as a Spaniard, Vives was thoroughly unwilling consciously to borrow or adopt from Moorish sources. He treats of 'souls' as distinguished from 'torpid things' (or the inorganic world), the distinction being founded on the power of self-movement. He is thus provided by the Aristotelian-Scholastic psychologists with (1) the *anima alens* of the plants, (2) the *anima sentiens* of zoöphytes, (3) the *anima cogitans* of birds and four-footed beasts and (4) the *anima rationalis* of man. The soul of man is the 'form' of which the body is the 'matter.' The human body therefore is a potentiality, which is actualized by the union with it of the soul. But this principle of the form giving actuality to the potentiality of suitable matter is characteristic of all life, *e. g.*, in plants, zoöphytes, birds, beasts—up to and including man. The lowest kinds of life, *e. g.*, plants, have the motions of nutrition, growth and decay. Animals, in addition, have sensation, and developments from sensations. Man combines all these stages, including the appetency which springs from sensation, cognition and reasoning. The soul of man is therefore an epitome of all lower life and also possesses psychical ingredients of its own. Hence, an investigation is necessary into the vegetative 'soul,' and the 'animal' soul, as well as the human soul. Thus the physical phenomena of nutrition, growth, decay, generation, sensation (in general) and the special senses (and the hierarchy amongst them), are passed in review, then 'interior cognition' including imagination, phantasia (into which angels, good and bad, insinuate themselves), the *sensus communis*, the cognitive judgment and reason. Vives then offers his defini-

tion of the soul[1] and makes the usual inquiry as to the seat of the soul, deciding, with Aristotle, that it 'informs' the whole body; though certain functions are localized, *e. g.*, the front part of the brain is the seat of *phantasia* and in the back part of the brain memory is localized, and so on.

The above topics comprise the contents of Book I. of Vives's 'De Anima.' Book II. is devoted to the rational soul and its faculties. Man has been created for eternal felicity and provided with the means for accomplishing it. This implicitly demands the intelligence to know the good, the memory to retain that knowledge, and the will to act it out in life. We thus have the trinity of the soul. Vives then describes in detail the functions of the *simplex intelligentia* (simple apprehension), memory and reminiscence, composite ideas, reason, judgment, mental ability and its individual varieties, speech, the method of learning (in which Vives inquires why there are so few learned people), knowledge, contemplation, will (in which occurs the discussion on its freedom),[2] on the mind in general, on sleep and dreaming, 'habit,' old age, length of life, death, and the immortality of the human soul.

In the third book of the 'De Anima,' the subject of which is the emotions or passions (affectus), Vives undoubtedly followed[3] St. Thomas Aquinas. We have seen that Vives regarded the intellect as supplying knowledge as to the good, which the will was to carry into effect. We are, therefore, prepared to find that 'passions' are looked upon in their relation to the supreme end to be achieved in volition. They are defined by Vives as 'the natural faculties of our soul by which we are carried towards the good and endeavor to avoid

[1] This follows substantially the teaching of Aristotle. Animam esse agens praecipium, habitans in corpore apto ad vitam.

[2] It is worth noting that Vives strongly protests against the doctrine current long after his age, that the will is controlled or influenced by the motions of the stars. The significance of the protest is only realized when we remember that Tycho Brahé, Kepler and even Galileo 'cast nativities.'

[3] Aristotle is out the question. As Mr. Hicks says Aristotle 'exalted the cognitive element, while his treatment of the emotions and the will is wholly inadequate even if the ethics and the rhetoric be called in to redress the balance.' *Aristotle de Anima*, p. LXXII. On the parallels between Vives and St. Thomas Aquinas see Roman Pade, 'Die Affectenlehre des J. L. Vives,' Münster i. W., 1893.

the evil.' The close relation, therefore, between psychology and ethics is evident. Vives's treatment of the passions, though based on St. Thomas Aquinas, is yet largely supplemented by his own introspection and observation.

Whilst thus he tends to emphasize the interest in the description and analysis of the separate emotions, and to give the result of his wide intercourse with men of very varied kinds, he has not been so thoroughgoing in his psychology of emotion in general. He does not offer an elaborated theory of the passions like Descartes and Spinoza, leading in the one case to a discussion of emotion in the abstract, and in the other to an *à priori* mathematically-based theory, but he is quite as comprehensive and at points shows depth of empirical interest, which place him in the direct line of continuity between the Scholastics and the modern descriptive school of psychology, in the treatment of the passions. Vives reduces all the passions to two, love and hate. All that stirs and stimulates towards the good, comes from the incitement of love, and all that stirs to evil, from the passion, in some form, of hate. Yet Vives describes fully and separately the passions: good-will, respect, sympathy, gaiety, hope, laughter, annoyance, scorn, anger, hatred, envy, jealousy, indignation, vengeance, cruelty, sadness, mourning, fear, shame, pride. Vives is strongly attracted to the Platonic treatises, especially the 'Phædrus' and 'Symposium.' The value of the third book, as with the other two, is in its empirical method, for it also contains numerous observations, personal illustrations based on Vives's own introspective experiences. The distinguished psychologist Harold Höffding[1] in the section on the psychology of the feelings, whilst discussing the topic of laughter, quotes Vives, who described himself as unable to refrain from laughter as he first tastes food after a long fast ('De Anima,' Bk. III[2]). And, again, Höffding recalls the observation of Vives, that what man expresses by laughter may be expressed by animals in other ways[3] (*e. g.*, by wagging the tail).

[1] 'Outlines of Psychology' translated by Mary E. Lowndes, 1891, pp. 291–2.
[2] 'Opera,' III., p. 469.
[3] 'Opera,' III., p. 470.

In his professedly psychological work—throughout the three books of the 'De Anima'—Vives discloses himself as the pioneer of the modern empirical method in psychology. But we only realize the full sense of conviction, which animated Vives in his use of the method of introspection and observation, when we further note in his other writings, his constant application of the same method in the ordinary affairs of life. He applied psychological principles to professional practice, to individual conduct, and particularly to the function of teaching. In other words, in practical affairs, he sought to introduce psychological precepts and methods, to create a habit of introspection which might be turned to use—to create an atmosphere of psychology, to think psychologically.

No writer of the Renascence period was so distinguished by his application of psychology to education, as Vives. In his 'Transmission of Knowledge' ('De Tradendis Disciplinis,' 1531) he is permeated with the desire to bring education to a psychological basis. We have seen that his account of the memory was an outstanding feature in his *de Anima*. But in dealing with the subject from an educational point of view, he is in accordance with the most modern of writers in pointing out that both quick comprehension and faithful retention in memory are helped by a right *arrangement of facts*. This, he adds, is just that art of memory 'which beasts are said to lack.' His rules for the cultivation of memory have not lost their suggestiveness. For instance, he says: "What we want to remember must be impressed on our memory *while others are silent*. We need not be silent ourselves, for those things which we have read aloud are often more deeply retained. . . . It is a useful practice to write down what we want to remember, for it is not less impressed on the mind than on the paper, by the pen. The attention is kept fixed longer by the fact that we are writing it down." "Great is the help to memory if reasons are associated with the matter taught." We have seen that Vives recognizes *interest* as a strong stimulus to the attainment of knowledge. He goes far in the direction of Herbart in the recognition of interest not merely as a means in the acquisition of knowledge but also

in the advocacy of a wider scope of interests as the outcome of our studies. Vives in his story of Charles Virulus the schoolmaster of Louvain is as modern in significance as Herbart himself. When a pupil's parent came to visit, especially to dine with him, he made a point of finding out what his visitor's work and interests were and *prepared himself carefully* to converse on matters familiar to his guest, and lead him to speak freely on what was best known to him. "He would thus hear in the briefest time details which he could scarcely have gleaned from the study of many years."

Education with Vives is not the preparation for a career, but the increase in practical wisdom of life and the preparation for moral excellence. In each school masters should meet four times a year and discuss the 'nature' of each boy and then apply the boy to those studies for which he seems most fit. The fruit of studies is not honor or money, but the culture of the mind—'a thing of exceeding great and incomparable value—that the youth may become more learned and more virtuous through sound teaching.' Boys should only at first be taken on trial in the school. The teachers are to determine who are fit and who unfit for learning. As Ascham maintained, Vives previously urged that the slow wit is usually the surest. The wonderful variety of dispositions in boys requires the closest attention of teachers, in 'choosing' scholars. Yet there is scarcely anyone who will not profit by being taught, if the right sort of teaching is given him. Probably no Renascence writer has taken so much notice of the problem of the feeble-minded, the deaf and dumb, the blind, as Vives, though naturally he has not been able to suggest the most effective lines of training in each case. But his firm grip of the principle of suiting instruction to the individual capacity puts him in the direction of perceiving the problem involved. Vives, again, sees clearly that the problem of education is essentially that of self-activity. He requires the pupil to keep paper notebooks, in which he gathers for himself the main materials of his own instruction. His notebooks must have divisions and heads, and be provided by himself with indexes. In these he must enter, under proper heads,

all he learns from teachers and books. In other words, he must largely *make his own text-books*. He presents a well thought out psychology of school punishment, and indeed his psychology of examinations, if we may so call it, is in advance of present-day methods, for, instead of pitting boy against boy (when he has emphasized the great variety of original mental capacity) he logically requires the *comparison of the boy with himelf at an earlier stage*. "Let scholars keep what they have written in earlier months, in order to compare it with that written at a later month, so that they may perceive the progress made, and persevere in the way of improvement."

Thus, whilst Vives sees the overwhelming importance of building up the art of pedagogy upon a sound psychological basis, he by no means limits the value of the application of psychology to the work of the schoolmaster. The knowledge of psychology is essential to all who have to deal with spiritual affairs. "The study of man's soul exercises a most helpful influence on all kinds of knowledge, because our knowledge is determined by the intelligence and grasp of our minds, not by the things themselves." The text-books for psychology recommended by Vives are the sacred writers of the Bible, and the three books of Aristotle's 'De Anima' (especially Books II. and III.). Other writers to be read are Alexander, Themistius, Timæus of Locris, and Plato's 'Timæus,' Proclus, Chalcidius, and of the Renascence writer, Marsilius Ficinus, who will act as guide for Plotinus. The physician, to Vives, is on the very borderland of nature-study and soul-study, and must be at home in both. But the psychological aspect must be present not only in his studies, but in his professional habits. He must himself "not be in infirm health, not pallid in countenance, so raising the suggestion put in the sacred Gospel: 'Physician heal thyself.' Further let the doctor be clothed neatly rather than sumptuously. At the first sight of his patient he will immediately take in his appearance and constitution, age and vitality. All necessary information he will gather in an urbane and affable fashion. He will listen with patience. . . . He will neither exchange views nor discuss with other physicians in the presence of patients, or of lay-people, who

know not which side to take. To do so, easily raises a 'certain despair' in them and a hatred against knowledge, 'which comes to be regarded as a matter of uncertainty.' "

In the case of the historian, wars and battles are to be regarded as 'cases of theft.' The historian should study peaceful affairs, trace the glory and wisdom of virtuous acts and note the disgrace of evil-doers. The wisdom of great statesmen, and those who have excelled in 'good arts,' philosophers, saints of the faith, and all that has been said in practical affairs should be studied with the full force of weighty intellect and judgment. For "it is unworthy to hand over to our memories historical actions due to our passions, and not also to study what took place as the outcome of the rational judgment." History discloses the essential nature of human beings, and discloses the manifestations of the affections and judgment of the human mind, in short, the subject has a distinctly psychological basis.

Again, the politician and economist have to study the dispositions and minds of the people. Herein is the predominant value of experience. "Sometimes old men converse with one another in an experienced way and allow youth to listen." This is a privileged method of study to youth, if they kept free from the company of cavillers and obstinate dialecticians. "For a man to be more anxious about achieving a dialectic victory than of discovering truth leads to the ruin of practical wisdom, as indeed Cicero has said." Vives points out the mental characteristics desirable in the administrator, and for the study of political philosophy recommends not only Plato and Aristotle and other classical writers, but also the 'Utopia' of Sir Thomas More and Erasmus's 'Christiani Principis Institutio.' Equally clearly the lawyer must be a psychologist. He must understand "the common nature of mankind, the views and customs of many kinds of people, especially of his own country. This is brought about by wide experience in seeing, hearing, observing things; through reading of the deeds of ancestors and varieties of changes which have befallen the state. Such men need *alert minds and keen judgments, so as to observe and to estimate circumstances, one by one.*"

In all these instances Vives's introduction of psychological observation bears a modern aspect, and affords illustration of the attraction which he felt towards the empirical standpoint and self-exercised thought on the environment rather than the older type of abstract, metaphysical explanation or discussion of the more ultimate foundation of psychological phenomena. The most marked characteristic of the early Renascence writers is the backward-looking concentration on the golden age of Roman and Greek culture and knowledge. In a notable passage[1] however we find Vives exclaiming: "The student should not be ashamed to enter into shops and factories, and to ask questions from craftsmen, and to get to know about the details of their work. Formerly, learned men disdained to inquire into those things which it is of such great import to life to know and remember. This ignorance grew in succeeding centuries up to the present . . . so that we know far more of the age of Cicero or of Pliny than of that of our grandfathers." His keen interest in the experiential side of psychology, therefore, infused itself into his whole outlook, in wishing to get an intellectual grip of the problems of the human mind, and its manifestations, in its relations to its own growth and development, and also in its actions and reactions, in connection with its environment. No doubt he believed that the glorious achievements and experiences of the past threw light on those questions, perhaps to a degree which has been lost in modern times, but Vives's psychological *attitude* towards life, in its present environment is clearly modern, rather than ancient or mediæval.

In England, Vives's name has fallen into undeserved oblivion. For the Spaniard of Valencia made his home in this country for portions of the year from 1523 to 1528, the year in which he had to withdraw from England, on account of his known adhesion to the cause of Queen Catharine of Aragon, who had such a belief in his abilities that she desired him to act as her advocate in the court, so adroitly constituted by Henry VIII to try her case. During his visits in England, Vives lectured on rhetoric at Oxford, where

[1] 'De Tradendis Disciplinis,' Book IV., Chapter 6. 'Opera,' VI., p. 374.

he was associated with Corpus Christi College. He was one of the friends of Cardinal Wolsey, and of Sir Thomas More. Yet, curiously, as we have seen, it is to a representative of Scotland, Sir William Hamilton, that Vives particularly owes his acknowledgment, in the last century, though another Scot, Dugald Stewart, perhaps even more fittingly brings out the modern aspect of Vives, in the striking passage:

"Of all the writers of the sixteenth century, Ludovicus Vives seems to have had the liveliest and the most assured foresight of the new career on which the human mind was about to enter. The following passage from one of his works[1] would have done no discredit to Francis Bacon's 'Novum Organum': 'The similitude which many have fancied between the superiority of the moderns to the ancients, and the elevation of a dwarf on the back of a giant is altogether false and puerile. Neither were *they* giants, nor are we dwarfs, but all of us men of the same standard,—and *we* the taller of the two, by adding their height to our own: Provided always, that we do not yield to them in study, attention, vigilance and love of truth; for, if these qualities be wanting, so far from mounting on the giants' shoulders, we throw away the advantages of our own just stature, by remaining prostrate on the ground.'"

[1] 'De Causis Corruptarum Artium,' Bk. I., Chap. 5. 'Opera,' VI., p. 39.

AN INVESTIGATION OF THE LAW OF EYE-MOVEMENTS

BY MILDRED WEST LORING[1]

The first investigation of eye-movements was made in 1826 by Johannes Müller (1). He stated that the eyes in their movements do not rotate about their long, *i. e.*, sagittal axes. He said: "I have convinced myself, while observing various points on the white of the moving eye, which were marked beforehand with ink, that the eye, through action of the oblique muscles, does not rotate about its long axis." In 1838, however, Hueck (2) demonstrated the compensatory rotation of the eyes around the line of sight, by observing that a given horizontal blood-vessel on the conjunctiva remained horizontal even when the head was inclined to the right or left. Burow (3), 1841, reached the same conclusion using his own paralyzed iris for the demonstration. But Ruete (5), in 1846, like Müller, got negative results, using after-images for a criterion. Donders, (7) 1848, showed his work to have been careless, and demonstrated the well-known principle, that the after-image of a vertical strip remains parallel to itself, with vertical and horizontal movements of the eyes, but with oblique movements, becomes itself oblique. He however formulated no law.

It remained for Listing to put the law concisely in the form now known as Listing's Law, although he did not prove it, or publish it himself. It first appeared in this form in an article by Ruete (11) in 1855, in which he says: "The principle of the mechanism of the eye can be expressed according to Listing in the following simple way; 'From the above-mentioned normal position of the eye which may be called the primary, the eye will be moved into any other secondary position by the coöperation of the six muscles, in such a way

[1] The results of this paper were obtained in the psychological laboratory of the University of Washington, 1912–13, under the direction of Dr. H. C. Stevens.

that this displacement can be represented as the result of a rotation about a definite axis different from the above three, which always passes through the center of the eye, and is perpendicular to the primary and secondary position of the optic axis, so that each secondary position of the eye stands in such a relation to the primary, that the rotation projected on the optic axis will equal zero.'"

Meissner (9), in 1854, was the first to use the method of double images, which he observed are not parallel for a given object with near or far fixation. The inclination of the two images he took as representing the torsion of the two eyes about the line of sight. Both Meissner (13), (1860) and Fick (12), (1858), investigated the subject by means of the change of orientation of the blind spot with eye-movements. Fick's results are irregular, and he says that the movement of the eyes is not a simple geometrical one, about a definite axis, as Meissner held, but that it is a physiological change of position, whose beginning and end only, we know. Meissner found his results to be similar to those of Listing.

The next important work on eye-movements was done by Wundt (14) in 1862. He used the method of after-images, and presented the theory that the eye rotates to such a degree as to allow it to take the desired position of the line of sight with the least muscular effort. He found a slight torsion inward for movements of the eyes vertically above, and torsion outwards for movements vertically below. Helmholtz (15), in 1863, denied the reliability of Wundt's hypothesis, owing to individual differences in muscle strength, and the general unreliability of the muscle sense. He preferred to state the facts in the form of Donder's law, which says: When the position of the line of sight in relation to the head is given, we have a definite and unchangeable amount of rotation.

Volkmann (16), in 1864, attacked the problem somewhat after the fashion of Meissner, using two rotating discs, one for each eye, on each of which was marked a diameter. These were rotated till judged parallel, for any given convergence, and the real torsion thus determined. The torsion as expressed by its effect on the two diameters placed vertical, was as follows:

Primary position	2°.21
30° above to the right	2°.74
30° above to the left	2°.92
30° below to the left	1°.31
30° below to the right	1°.40

This small amount of variation accounts, he says, for the difficulty of using after-images.

Helmholtz (17), 1866, using after-images, got the following results:

A. Turning the eyes to the right above or left below
1. After-image of horizontal line is turned to the left.
2. After-image of vertical line is turned to the right.

B. Turning the eyes to the left above or right below
1. After-image of horizontal line is turned to the right.
2. After-image of vertical line is turned to the left.

In 1868, Hering (18) published his results on binocular vision. In the main he agrees with Helmholtz. He worked with (*a*) after-images, using parallel lines of sight, observing the customary phenomena of Listing's figure, (*b*) double images, using convergent lines of sight. His general conclusion was that Listing's Law holds for parallel lines of sight, but not for convergence lines of sight. According to him, then, Donder's law is invalid, inasmuch as it maintains a constant torsional value for a fixed position of the line of regard in relation to the head.

Le Conte (20), in 1881, published a severe criticism of Helmholtz's version of Listing's Law, and framed it in terms exactly opposite to those of Helmholtz. The whole occasion of the difference of conclusion rests, as Le Conte shows, on the fact that he has used the normal spherical surface for projection of the after-images, while Helmholtz stated the law for projection on a plane surface. Le Conte denies all real rotation about the sagittal axis. It is only apparent, he says, and attributes it to a rotation about some axis perpendicular to the sagittal axis, which, because we observe all the movements of a spherical surface like the eyeball from one point of view.

we interpret as rotations about the line of sight. So Le Conte concluded there was no torsion with parallel lines of sight; but for convergence he finds not only an apparent but a real torsion about the line of sight.

One of the most recent studies on eye-movements has been made by Bernice Barnes (23) in 1905, at the University of Michigan. Her apparatus which she calls a torsio-meter, consisted of 'an iron arc of 180°, one meter in diameter, mounted on a standard, so that its center may be directly in front of the eye of the subject, who is seated before it.' The arc could be swung into horizontal, vertical and oblique positions. It was fitted with a telescope which was directed at the observer's eye. The latter seated himself in front of the arc, with head firm; a thread stretched between the extremities of the arc served to determine the center of the arc, at which point the observer kept the pupil of his eye. The cross-hair in the telescope was set in coincidence with a line on the iris, which reading was taken as zero. Then the telescope was changed in position, and the eyes were again directed at the telescope. The cross-hair in the telescope was set again on the same line on the iris. In this way any real torsion of the eye about its sagittal axis was determined. Miss Barnes found real torsion for every position different from the primary position, even with parallel lines of sight, a conclusion very different from Listing and his successors'. Her chief conclusions are these:

1. There is a contradiction between Listing's and Donder's Laws for torsion in eye-movement.

2. Experiments by the after-image method seem to confirm Listing's Law. But there are two sources of error—inaccuracies in measurement, and false torsion, which it is difficult to make allowances for.

3. More accurate direct measurements show that there is always torsion with rotation, and the amount of torsion is proportional to the amount of rotation.

4. Donder's law holds.

Miss Barnes's work is interesting for the reason that she found, even with parallel lines of sight, an invariable real

torsion with movements away from the primary position in any direction whatever. Yet a closer analysis of her results shows certain ambiguities.

1. There is nothing in her article to show which eye of the observer was used in the experiment, or whether both were used indifferently.[1] This is an important point, for it is possible that the two eyes undergo torsion symmetrically, but in opposite senses, when the lines of sight are either parallel or converged. An adequate interpretation of her results is impossible without a knowledge of this point.

2. Her results show that there are some regions, for instance between 0° and 30° to the right above, where the sense of the torsion changes, for at 0° it is to the right and at 30° it is to the left. This must indicate one of two things; (*a*) that somewhere between 0° and 30° there is no torsion at all, this position being at the point where the direction of the line of sight changes from right to left. This point, mathematically speaking, could be called the point of inflection. (*b*) Otherwise the phenomenon must indicate a region in the field of vision such that, if one directs the line of sight there, the sense of the torsion depends on chance, that is, it is as likely to be to the right as to the left and cannot be predicted. Since Miss Barnes denies any direction in which there is no torsion, the first alternative is for her out of the question. She would of necessity have to accept the second. But this, however, violates Donder's Law, which Miss Barnes upholds, inasmuch as the law mentioned demands a constant torsional value for every given position of the line of regard.

The only conclusion that can be drawn from Miss Barnes's work is that further investigation is necessary in the above-mentioned regions of the fields of vision, using each eye, and comparing the results for the two eyes.

Wichodzew (24), 1912, has recently contributed to the subject. He has used the size of the field of vision (both monocular and binocular) to determine the influence on eye-movements of the inclinations of the head to the shoulder. His main conclusions are:

[1] I have since learned that only one eye of the subject was used, but which, is not recalled by the author.

1. There is a compensatory torsion (Raddrehung) of the eyes about the sagittal axis, which causes a change in the mutual relation of the fixation points of the eye muscles and so hinders the eye movements.

2. The capacity of the eyes for symmetrical torsion about a sagittal axis increases with inclination of the head to the right or left shoulder. This increase might be explained as a stimulation by the compensatory torsion to the innervation of the muscles, which turn the eye about the sagittal axis.

The work of Wichodzew, like that of his predecessors, still leaves the question of the sense of the torsion for each individual eye still undecided. Besides there is no reason for accepting a priori that the movements of the eyes are the same when the fixation point is kept constant and the head rotated, as when the head is kept constant and the fixation shifted.

Repetition of Previous Experiments by Hering and Helmholtz

Hering.—When two parallel strips, at interocular distance, are looked at with the eyes in the primary position, the two strips are seen as three, all parallel. But as soon as the fixation point approaches nearer than infinity, the three break into four, the inner one making two images not parallel but converging at the top. This is due to torsion of the eyeball, which increases as the fixation point is brought nearer the observer. Likewise, when the eyes are shifted horizontally to right or left, the convergence of the two middle strips increases, the greater the shift of the eyes. The purpose of this experiment was to measure the amount of torsion and to measure individual differences in the amount of torsion.

Essentially the same apparatus and procedure was used as that described by Hering (18). Likewise the results obtained agree with Hering.

1. The amount of torsion of the eyes, as measured by the convergence of double images of two straight lines, increases directly as the amount of convergence of the eyes increases, from an apparent minimum of zero to the maximum with maximum convergence.

2. The torsion of the eye increases as the point of fixation is directed to a greater distance horizontally either left or right of the primary position.

3. Keeping the fixation point constant, a bending of the head forward increases the amount of torsion; raising the head decreases the amount of torsion.

Helmholtz.—(*a*) It is an established fact that we do not see all actual straight lines as straight. In fact, if we look at a line which is really straight, thus ———————— with parallel lines of sight and a card held in front of the nose in the median plane of the body so that each eye sees but half the line, it will not seem straight but broken, the two arms converging thus (allowing for much exaggeration):

If now by some device we compensate for this apparent brokenness, the line must actually be made thus:

Now if the eyes are converged to the middle point of the line, where the card meets the line, the line which had been made straight apparently, seems broken again thus:

And to compensate for this additional apparent slant of the arms of the line, the two arms of the actual line must be bent toward one another downward, still more, thus:

The apparent convergence of the two arms of the line therefore increases as we pass from an infinite fixation point to one very near the eyes. This is due to actual torsion of the eyeball about its sagittal axis, the right eye turning to the left and the left eye turning to the right, from the point of view of the observer.

The apparatus, based on Helmholtz's idea (17), consisted of a strip of cardboard 10 × 1.5 cm., fastened horizontally at one end by a screw to a vertical screen of cardboard. Along the length of the strip, which was at the level of the eyes, was drawn a black line which was extended through the point of rotation to an equal horizontal distance on the screen. This gave a horizontal line whose apparent brokenness when fixated under certain conditions, with a vertical screen perpendicular to the face in the median plane, could be compensated for by rotating the strip, and so give a measure of the rotation of the eyes.

The results indicate that with any given fixation point, there is a torsion of the eyes, the left eye to the right, and the right eye to the left, from the point of view of the observer himself. This result agrees with that obtained in Hering's double image experiment above, and with the results of our original experiment below.

Helmholtz.—(*b*) It has been shown that if the head is held in the primary position, and a colored cross, with vertical and horizontal arms (Listing's figure), be located on a vertical screen with the intersection point at the middle point of the line joining the points of intersection of the lines of sight of the eyes with the screen, the shifting of the eyes to any oblique position when an after-image has been developed with head kept fixed, results in the following distortions of the arms of the cross (17):

1. Turning the eyes to the right above or left below
   After-image of horizontal arm is turned to the left.
   After-image of vertical arm is turned to the right.
2. Turning the eyes to left above or right below
   After-image of horizontal arm is turned to the right.
   After-image of vertical arm is turned to the left.

According to Listing and corroborated by Helmholtz, if the eyes are turned straight above or below, or directly to right or left, the after-images of horizontal and vertical arms remain horizontal and vertical.

A much modified form of Helmholtz's apparatus was

employed. It consisted essentially of a black strip rotating at its middle point against a white screen. The after-image of the black strip could be directed to any angle of any quadrant of the field of vision at a fixed distance from the point of rotation. The torsion of the eye as determined by a torsion of the after-image was measured by making the strip parallel to the after-image, and reading the angle of torsion directly.

The results show that there is a torsion for all angles of oblique fixation, for the right eye to the left and the left eye to the right, from the point of view of the observer. This is contrary to Listing, Helmholtz and Sanford in that they deny torsion for the horizontal and vertical, and Sanford also for 45°. The results show too that the method of after-images is capable of showing the vertical and horizontal torsion, which Miss Barnes denied.

Experiment

Problem.—It was the purpose of this investigation to see whether or not the long-debated question of torsion of the eyeball around the sagittal axis could be settled by means of a very carefully made apparatus which would detect any rotation ordinarily unobserved by other methods of investigation. To that end, recourse was had to the principle of the method of Johannes Müller, namely the observation of a fixed line upon the eye when that eye is placed in different positions in the field of regard. The fixed line of the eye was one of the radial striæ of the iris. A description of the apparatus, by which the observation was made, follows.

Apparatus.—The apparatus consists of a large, vertical, semicirculcar iron arc, one meter in radius, attached solidly to a heavy brick chimney in the dark room of the psychological laboratory. By means of a galvanometer, it was determined that the vibration of the chimney is so slight as not to overcome the inertia of the apparatus. The iron arc, which was turned in a lathe to insure smooth surfaces, is 5 cm. wide and 2.5 cm. thick, and is graduated into arcs of 10°. The whole arc rotates in a horizontal direction about its extremities, but its points of rotation are shifted 7.5 cm.

to the right by means of iron plates, to accommodate for the eccentric position of the telescope which is attached to the arc. In this way, the vertical cross-hair of the telescope coincides with the axis of the arc. The upper center of rotation has on it a horizontal, circular protractor, 7.5 cm. in radius, graduated in degrees, to measure the amount of rotation of the arc. Attached to the right side of the arc is a transit telescope, with a magnifying power of 34 times, at the distance from the telescope to the middle point of the arc. The magnifying power was determined as follows:

At a distance of one meter from the telescope a meter stick was placed in a vertical position. A rider, made of a narrow band of paper, 1.5 cm. wide, was freely movable along the length of the meter stick. The experimenter then looked at the rider through the telescope with his left eye, and directly at it with his right eye, and adjusted the rider, so that the upper edge of it, magnified in the telescope, coincided with the upper edge of it, unmagnified, as seen by the right eye. With the images of the left and right eyes thus superimposed, the experimenter observed the length of the magnified rider on the unmagnified scale of the meter stick. The left eye image of the magnified meter stick and right eye image of the rider, were inhibited. The quotient then of the magnified length of the rider and its unmagnified length represents the magnifying power of the telescope.

Magnified length of the rider.....51.5 cm.
Unmagnified length of rider..... 1.5 cm.
Magnifying power.....51.5/1.5=34.3 times

This method was checked by the method of focal lengths. The magnifying power = F/f, where F is the focal length of the objective and f the focal length of the eye-piece. Measurement of the focal lengths of the telescope showed:

Focal length objective, F.....26.5 cm.
Focal length eye-piece, f..... .8 cm.
Magnifying power.....26.5/.8 =33.1 times

The telescope of the apparatus is detachable and can be slid along the arc to any desired position and fastened there. It is directed inward, perpendicular to the arc.

Exactly at the middle point, between the two points of rotation of the arc, suspended by means of an iron support, extending out from the brick chimney, is a small brass ring, 3 cm. in diameter, permanent in position, at which the observer places his eye. Also from this same support is attached a head rest. It is made of an iron band 27 cm. in diameter surrounding the observer's head at the temples, and supported by four vertical iron strips adjusted in length vertically, which at a distance of approximately 12 cm. above their attachment to the iron band, bend inward to a common point, which is the center of support. Screws, with flat discs at their extremities, point inward through the iron band, which are adjustable to accommodate differences in size of head of observers. The chin is supported by a U-shaped piece of iron, swinging from the iron band so that it hangs about in the plane of the brass ring. It too is adjustable vertically by means of screws.

The head rest can be set for either right or left eye position. The observer places his head in the iron band, with the eye exactly in the center of the ring; the screws are tightened to fit the head, the band is lowered or raised to accommodate his height, the chin rest is adjusted likewise and then the head is so firmly fixed that there is no error from head movements.

The telescope is attached at about its middle point to the right side of the arc, and is adjustable in three planes, vertically, horizontally, and circularly about its long axis. It is equipped with vertical and horizontal cross hairs to obtain accurate settings. A vernier, which reads to 0′.5 is attached to read the circular rotation. It is to be noticed that since it is necessary practically to have the telescope fastened to one side of the arc, the points of rotation of the arc have been shifted an equal amount in the same direction. By dropping a plumb line from the upper point of rotation, the center of rotation of the arc was obtained, at which point the center of the brass ring was placed and marked by two perpendicularly intersecting threads, the vertical one being determined by the plumb line. The telescope was then set in a horizontal position at the zero point on the arc, which is half way between

its extremities, and the intersection of the cross-hairs of the telescope coincides with the intersection of the threads on the brass ring. In this way the points of rotation of the arc, the center of the brass ring, and the intersection point of the cross-hairs of the telescope all lie in a plane, parallel to the plane of the arc. With a new position of the telescope on the arc, or a rotation of the arc, the horizontal and vertical cross-hairs of the telescope still coincided with the horizontal and cross-threads passing through the center of the brass ring. This statement must be modified. Since the apparatus is to measure rotation in a vertical plane, perpendicular to the plane of the arc, the aim was so to construct it that, with the arc and telescope in a given position, a change in position of the arc, or of the telescope on the arc, the vertical and horizontal cross-hairs would still be horizontal and vertical. By making the axis of rotation of the arc absolutely vertical, placing the center of the brass ring precisely at the center of the diameter of this axis, and since the arc was constructed as true as possible, it was found that the vertical and horizontal cross-hairs remained so with various positions of the apparatus. There was but one variation in the absolute trueness of the instrument. When, for instance, the arc and the telescope were both placed in the zero position and the vertical cross-hairs set coincident with the vertical cross-thread of the brass ring, and then the arc was rotated to right, the thread and cross-hair, though both remained vertical, were no longer coincident but were laterally displaced. Since they were parallel, by adjusting the screw controlling the horizontal shift of the telescope, they could be brought into concidence again. In this operation, the cross-hair was not rotated from its vertical position, so that whenever a rotation of the cross-hair was necessary in the actual experimentation, to cause coincidence, it indicated, not imperfection in the apparatus, but a rotation of the line which the cross-hair of the telescope was being set upon. This was the intended function of the apparatus. This parallel shift was due to the size of the apparatus and the high magnifying power of the telescope, but did not amount at a maximum to

more than two or three turns of the small screw, controlling the horizontal adjustment. The same displacement occurred in a vertical direction but to a much less degree. Two electric lights served for illumination. One light was attached to the arc by a jointed arm and could be slid along the length of the arc. It was used to illuminate the vernier; the other was suspended from a horizontal arm pivoted to the top of the head rest, and illuminated the eye of the observer. A frosted globe was used in this lamp, to lessen the fatigue of the eye, and to give a more diffuse illumination.

Procedure.—When the observer was seated with his head in the head rest as indicated above, the arc and telescope were placed in the primary or zero position; that is, the arc was swung to a zero position on the dial, and the telescope set horizontally on the arc at its middle or zero point. The observer sat with his left eye at the brass ring and the plane of his face perpendicular to the plane of the arc, so that looking straight ahead of him, his eye was fixed on the telescope. A small spot of white paint was placed at the middle point of the objective to facilitate fixation. Next, the operator examined the eye through the telescope, and selected on the iris a line or set of two points upon which to set a cross-hair of the telescope. The same line was not used through the whole experiment, inasmuch as it might have an unfavorable location for observation with certain oblique positions of the eye. This initial reading on the vernier was taken as the zero reading. Whenever a new line was taken on the iris, and with each sitting of the observer, a new zero was taken.

With the telescope at zero position still, the arc was swung 15° to the right. The head of the observer was not moved; in fact, its position was kept constant throughout a sitting. The eye, however, was turned horizontally in its orbit to the new position of the telescope and fixed again on the white spot. The operator reëxamined the eye, set the cross-hair on the experimental line of the iris, and read the vernier. The difference between the first and second readings represented the angle of torsion of the eyeball. Similarly angles of torsion were obtained for 15° to the left and 30° to both right

and left. The arc was then returned to the zero position of the dial, and the telescope was then moved along the arc to a position 15° above the zero, or middle point of the arc. The reading was taken here, and again the arc was swung 15° to the left and to the right and also 30° to the left and to the right, and readings taken. The difference of the readings in any position from the reading of the zero, represented the torsion for that position. Observations were made upon both right and left eyes. As summary of the positions of the eye in which readings were taken is as follows:

Telescope.	Arc.
I. *30° above*	*30° left—15° left—zero—15° right—30° right*
II. *15° above*	*30° left—15° left—zero—15° right—30° right*
III. *Primary position*	*30° left—15° left—zero—15° right—30° right*
IV. *15° below*	*30° left—15° left—zero—15° right—30° right*
V. *30° below*	*30° left—15° left—zero—15° right—30° right*

In spite of the accuracy of the instrument, certain unavoidable errors arose in taking data. These were:

1. Certain reflex pupillary expansions and contractions even with a constant intensity of illumination. This changed the size of the iris and therefore the direction of the line set upon, in spite of every care to have the line a true radius of the iris.

2. Nystagmoid movements of the eyeball, entirely involuntary, due to reflex contractions of the eye-muscles, as a result of continued fixation.

3. A marked change in the pattern of the iris, owing to torsion. Sometimes it was difficult, in a new position of the telescope, to recognize the line to be set upon, owing to certain twistings in the pattern, which, while not very extended, yet interfered markedly with an accurate setting of the cross-hair.

Results.—1. Each number in Table I. (*a*) and (*b*) represents the average of five observations in that position.

2. In Table I., the column headed "Position," the sign + before a parallel indicates a position of the telescope above the zero parallel on the arc, the sign −, a position below the zero parallel on the arc; the sign + before an angle in the subgroup (+30°, +15°, 0, −15°, −30°) underneath a parallel, indicates a position of the arc to the right of the zero on the

Table I

Position	(a) Left Eye						(b) Right Eye					
	Torsion			Mean Variation			Torsion			Mean Variation		
	Observer (1)	Observer (2)	Observer (3)	Observer (1)	Observer (2)	Observer (3)	Observer (1)	Observer (2)	Observer (3)	Observer (1)	Observer (2)	Observer (3)
+30° parallel												
+30°	− 0° 0′.8	+12° 4′.0	+ 5° 44′.4	0° 10′.4	0° 12′.9	0° 17′.6	+4° 43′.6	+ 6° 31′.8	−2° 21′.4	0° 16′.2	0° 12′.8	0° 11′.1
+15°	− 0° 5′.8	+ 8° 16′.4	+ 7° 7′.0	0° 42′.8	0° 13′.4	0° 14′.1	+5° 9′.4	+ 1° 40′.8	+4° 16′.2	0° 18′.8	0° 12′.9	0° 16′.7
0°	− 4° 0′.0	+ 2° 42′.6	− 1° 45′.6	0° 53′.2	0° 13′.1	0° 10′.8	+2° 41′.4	− 2° 20′.6	+6° 4′.6	0° 14′.0	0° 20′.5	0° 25′.8
−15°	−12° 3′.6	+ 2° 3′.6	− 9° 14′.8	0° 17′.3	0° 14′.5	0° 19′.8	−3° 36′.4	− 6° 46′.4	−2° 56′.2	0° 20′.6	0° 24′.0	0° 17′.6
−30°	−17° 18′.2	− 6° 1′.6	−15° 4′.8	0° 20′.6	0° 10′.8	0° 22′.9	−7° 27′.8	− 7° 18′.0	−4° 1′.8	0° 20′.7	0° 8′.6	0° 6′.6
+15° parallel												
+30°	+ 2° 50′.0	− 2° 10′.2	− 2° 16′.4	0° 17′.6	0° 14′.6	0° 10′.2	+6° 2′.8	+ 1° 42′.2	+2° 45′.6	0° 13′.7	0° 13′.6	0° 12′.6
+15°	+ 3° 27′.9	− 4° 34′.6	− 1° 49′.4	0° 42′.7	0° 16′.1	0° 3′.1	+4° 14′.0	− 0° 21′.0	+3° 24′.4	0° 10′.3	0° 14′.0	0° 19′.1
0°	+ 2° 29′.6	− 3° 42′.2	− 0° 41′.2	0° 18′.1	0° 17′.0	0° 22′.3	+2° 55′.0	− 6° 33′.2	+1° 24′.0	0° 12′.1	0° 11′.4	0° 30′.4
−15°	− 3° 44′.8	− 5° 48′.6	− 4° 5′.4	0° 31′.7	0° 19′.3	0° 16′.0	+0° 34′.0	− 3° 15′.6	−1° 5′.8	0° 15′.5	0° 22′.2	0° 31′.7
−30°	− 5° 41′.4	−10° 56′.6	− 8° 3′.8	0° 55′.6	0° 15′.7	0° 6′.5	+0° 58′.2	− 2° 24′.0	−1° 48′.6	0° 21′.9	0° 11′.2	0° 68′.4
0° parallel												
+30°	− 0° 58′.8	− 4° 10′.6	− 2° 19′.2	0° 16′.5	0° 12′.4	0° 17′.5	−2° 58′.2	− 3° 12′.8	+0° 52′.4	0° 20′.0	0° 11′.7	0° 15′.0
+15°	+ 0° 20′.2	− 2° 18′.6	− 0° 41′.4	0° 17′.7	0° 20′.0	0° 9′.7	−1° 8′.8	− 1° 12′.6	−3° 4′.4	0° 14′.5	0° 22′.3	0° 13′.6
0°	0	0	0	0	0	0	0	0	0	0	0	0
−15°	− 5° 4′.6	− 2° 16′.0	− 1° 11′.8	0° 26′.6	0° 13°.8	0° 9′.2	+0° 31′.0	− 2° 16′.0	+0° 10′.0	0° 15′.5	0° 14′.4	0° 27′.5
−30°	− 3° 47′.4	− 3° 16′.2	− 2° 7′.4	0° 23′.4	0° 8′.2	0° 15′.5	+0° 59′.0	− 5° 21′.0	+0° 9′.2	0° 15′.2	0° 7′.6	0° 10′.9
−15° parallel												
+30°	− 8° 37′.4	−15° 54′.4	− 3° 42′.6	0° 12′.9	0° 21′.4	0° 14′.4	−6° 36′.6	−11° 19′.3	−3° 8′.0	0° 17′.2	0° 16′.4	0° 7′.6
+15°	− 2° 32′.8	−10° 18′.7	− 2° 28′.4	0° 18′.9	0° 20′.7	0° 17′.4	−0° 2′.6	− 8° 55′.7	+4° 21′.6	0° 6′.8	0° 12′.3	0° 17′.0
0°	− 1° 53′.0	− 3° 14′.4	− 2° 25′.0	0° 23′.9	0° 12′.9	0° 20′.0	+0° 41′.0	− 5° 5′.5	−0° 12′.0	0° 17′.6	0° 9′.8	0° 10′.0
−15°	− 1° 46′.0	− 1° 53′.2	− 4° 10′.2	0° 27′.7	0° 18′.4	0° 6′.5	+0° 41′.0	− 4° 57′.2	−1° 17′.2	0° 22′.3	0° 16′.8	0° 29′.7
−30°	+ 0° 26′.0	+ 0° 10′.4	− 4° 29.2′	0° 8′.3	0° 13′.2	0° 21′.1	+1° 16′.4	− 2° 57′.6	−4° 6′.8	0° 26′.4	0° 11′.9	0° 19′.0
−30° parallel												
+30°	−17° 38′.0	−16° 58′.6	+ 9° 32′.6	0° 54′.1	0° 16′.0	0° 46′.9	−7° 19′.4	− 8° 14′.4	+9° 55′.4	0° 25′.4	0° 20′.7	0° 26′.2
+15°	−10° 11′.8	−10° 48′.6	+ 0° 12′.2	0° 37′.8	0° 12′.0	0° 15′.4	−1° 25′.4	− 4° 11′.8	+5° 55′.6	0° 22′.2	0° 22′.4	0° 27′.2
0°	− 6° 15′.6	− 6° 20′.4	− 0° 49′.0	0° 30′.0	0° 14′.6	0° 33′.4	−3° 20′.0	− 4° 21′.0	+2° 5′.0	0° 18′.6	0° 10′.8	0° 16′.1
−15°	− 4° 16′.0	+ 1° 41′.8	− 8° 20′.6	0° 26′.9	0° 21′.1	0° 19′.3	+4° 54′.6	− 1° 42′.8	+2° 20′.4	0° 16′.6	0° 22′.9	0° 18′.8
−30°	− 0° 33′.6	+ 2° 15′.2	− 5° 1′.4	0° 28′.2	0° 25′.9	0° 20′.6	+9° 40′.4	+ 4° 56′.8	+9° 33′.4	0° 29′.1	0° 35′.0	0° 25′.6

dial, the sign − before an angle indicates a position to the left. For instance $\begin{matrix}+15^\circ \\ -30^\circ\end{matrix}$ parallel indicates a position where the telescope has been placed 15° above the zero or middle point of the arc, and the arc rotated 30° to the left of the zero on the dial. These signs are taken from the point of view of the experimenter, who looks through the telescope.

3. In the column headed "Torsion," the sign + indicates torsion of the eye to the right from the point of view of the experimenter; the sign − indicates torsion to the left.

Conclusion.—I. The results are to some extent irregular, both as to the amount of torsion and direction.

(*a*) As the eye travels from the primary position to scucessively more oblique positions, there seems to be a tendency for the angle of torsion to increase, but some exceptions occur. Likewise a movement of one eye to a given position does not give the same amount of torsion as the movement of the other eye through the same angle. Similarly, the sense of the torsion does not remain constant for one eye in all cases, and the torsions for the two eyes for movements through similar angles is not always opposite or always the same.

(*b*) There is a predominance of torsion to the right of the right eye, and to the left of the left eye, from the standpoint of the experimenter. Out of 24 positions for each eye away from the primary position, for each observer we find the following:

	Torsion of Right Eye		Torsion of Left Eye	
	To the Left	To the Right	To the Left	To the Right
1	9 cases	15 cases	19 cases	5 cases
2	20 cases	4 cases	17 cases	7 cases
3	10 cases	14 cases	20 cases	4 cases

It will be seen that for the left eye, the great predominance of torsion is to the left for all three observers, and that for the right eye, the predominance is to the right for two of the three observers.

II. The most definite conclusions consist then mainly in the two following observations:

(*a*) A tendency for the torsion to increase as the eye passes from the primary position to successively more oblique positions, including horizontal, vertical and 45° meridian.

(*b*) A predominance of torsion of the left eye to the left, and of the right eye to the right, from the point of view of the experimenter.

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VARIABILITY IN PERFORMANCE DURING BRIEF PERIODS OF WORK

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Numerous researches have been carried on to show the effects of working for long periods of time or with extremely difficult tasks. The effects, known as fatigue, may show themselves in a decrease in the quantity or quality of the product. Analogous changes in the character of the performance, which have scarcely warranted the name of fatigue as the term is ordinarily used, appear in extremely brief periods of mental work. For instance, Professor Woodworth in a recent paper,[1] showed some changes in speed of performance which take place during the naming of only ten simple colors. The present report contains a brief series of records showing a variation in performance in tasks, none of which lasted as long as one minute.

In some of the previous work on fatigue one finds references to the effects of brief work periods, although in few of them did the matter receive much attention. Voss[2] made detailed studies of an hour's work in addition, taking time for each separate problem. He concludes that practice tends to increase the number of rapid additions and fatigue the number of slow additions. The procedure followed made it impossible to separate the practice and fatigue effects for the short periods measured. Hylan and Kraepelin[3] measured the variation in adding one-place numbers in five-minute periods. They concluded that mental work lasting for only

[1] An unpublished paper read before the New York Branch of the American Psychological Association at Princeton, N. J., Feb. 23, 1914, entitled 'The Work Curve for Short Periods of Intense Application.'

[2] George von Voss, 'Ueber die Schwankungen der geistigen Arbeitsleitung,' *Psychol. Arbeit.*, Vol. 2, pp. 399–449.

[3] Hylan, J. P., and Kraepelin, E., 'Ueber die Wirkung kurzer Arbeitszeiten,' *Psychol. Arbeit.*, Vol. 4, pp. 454–495.

five minutes produced an appreciable amount of practice and fatigue. The ratio of the two factors differed for different individuals. In the work of Arai[1] on the multiplication of a series of two four-place numbers the time for each problem was measured. In the author's conclusions (p. 93) we note that "the difference between the time taken for one example and that taken for another is greater in the second half than in the first half of the curve. This fact together with the evidence of introspection of the subject suggests that fatigue not only causes decrease of efficiency, but also loss of the subject's control over herself. For this reason the subject tends to occasionally relax her original standard of effort." In this experiment the subject had passed the stage where practice was an important factor.

In the experiment to be reported two time records were taken for each task, one at the completion of the first half and the other at the end of the task. The time was taken with a stop watch in units of one fifth of a second. From these two records one may compare the performance during the first and second halves of the work. Speed was the only variable factor, for the experimenter announced all errors and these were corrected by the subject. It is to be noted further that the subjects were trained in all of the tests, and that practice effect was thus practically eliminated. If this were not the case it is quite possible that the improvement by practice within a single test might have clouded the results. An intensive study was made upon two subjects, each test being repeated from 60 to 70 times after the preliminary practice.

Four tests were used, two of them being taken from the Woodworth and Wells[2] monograph on association tests. These were the color naming test and the number checking test, called the cancellation test in this report. Only half of the regular number checking blank was used for one test, so that the halves measured were really fourths of the whole

[1] Arai, Tsuru, 'Mental Fatigue,' Teachers College, Columbia University Contributions to Education, No. 54, 1912.

[2] Woodworth, R. S. and Wells, F. L., 'Association Tests,' Psychological Monograph, 1911, No. 57.

blank. The numbers 3 and 5 were used alternately for cancellation. The third test was the opposites test and consisted of 50 words, the opposites of which were to be named. They were about equal in difficulty to the moderately difficult series of Woodworth and Wells. The fourth test was the addition test and consisted in adding 17 to each of 50 two-place numbers, ranging from 20 to 80 with all of the combinations containing zero being omitted. In every case the test material was divided into two parts by a line, so that the subject could see the limits of the first and second half. The subject's work, however, was continuous from the beginning to the end of the test.

The tests were repeated approximately five times each day, with an interval of several hours between any two, thus avoiding cumulative fatigue. All of these tests involve the process of association and the connection between situation and response was in each case partially established in the preliminary tests. It is to be remembered that the test material was always the same, the only difference being the order in which the stimuli were presented. There were four possible changes of order in the color naming test, by rotating the color blank 90 degrees at each trial; four in the number checking test, by using different halves of the test and either one of the numbers 3 or 5 for cancellation; ten in the opposites and addition tests by means of ten cards containing the same numbers in a random position on each.

The data of the experiment are presented in the accompanying table. The third column of the table shows the number of cases from which the calculations are made for each test. The fourth column gives the average speed for the first half of the test in terms of seconds. The fifth column gives the gross differences between the first and second halves. In every case the first half was subtracted from the second half, so that the absence of minus signs indicates greater speed in the first half. The sixth column shows the reliability of these differences in terms of the probable error of the difference. The seventh column shows the difference in terms of per cent. of the time of the first half. The column of gross

differences shows that in every test and for both subjects the first half is quicker than the second half, while the following

RELATION BETWEEN SPEED OF PERFORMANCE IN THE FIRST AND SECOND HALF OF THE TESTS.

(In every case the first half is subtracted from the second half, so that absence of minus signs indicates that the first half is faster.)

Test	Subject	No. Cases	1st Half	Gross Diff.	P. E.	Per Cent. Diff.
Opposites.....	*T*	59	16.2	1.9	0.1	11.7
	P	67	12.9	1.7	0.1	13.2
Addition.....	*T*	70	28.5	3.3	0.3	11.6
	P	67	18.4	1.1	0.2	6.0
Color Naming.....	*T*	63	20.0	1.9	0.2	9.5
	P	67	17.0	1.0	0.2	5.9
Cancellation.....	*T*	61	23.8	2.2	0.2	9.2
	P	60	20.3	0.7	0.2	3.4

column shows that the differences are reliable. The data when examined in detail and when treated in several other ways, show only one interesting exception. When the tests are grouped according to the time of day they were performed, namely 9.30 A. M., 1.30, 3.30 and 5.30 P. M., it appears that in the case of the cancellation test and at the 9.30 period both subjects were on the average slower in the first half than in the second half of the test. No explanation for this peculiar difference is suggested. The data treated in this fashion show only one other case where the first half is slower and here the negative quantity is less than its probable error.

The last column in the table shows the per cent. of difference in favor of the first half. It should be considered merely as indicating differences between *halves* in the tests, for only two subjects are not sufficient to establish differences of this type among the tests used. If the tests are grouped, one can say roughly that in speed of performance subject *T* was about 10 per cent. less efficient, and subject *P* about 7 per cent. less efficient in the second thirty seconds of the work than in the first thirty seconds.

A series of ten questions was given to each subject at the end of the series of tests to determine if introspection could throw any light upon the results. For instance, the

subjects were asked if they noticed any difference in speed, ease of performance, number of errors, etc., in different parts of the tests, and if so whether they could account for such changes. A separate series of questions was given for each of the four tests. Both subjects felt that they made more errors, and hesitated more in the second half than in the first. In both subjects there was consciousness of being slow with a feeling of inability to make the mind work any faster. The speed was probably judged by the number of errors and hesitations that occurred, according to the answers to some of the questions. The mistakes were attributed not so much to lapses of attention as to just getting tired. Neither subject reached the stage, during the course of the experiment, where the task became automatic, *i. e.*, where he no longer felt as though he were putting forth a great deal of energy.

In drawing conclusions from these records it is necessary to keep the specific conditions in mind: (1) The test material was always the same, except for the difference in arrangement. (2) The process involved in the reaction to the situations was mainly the recall of previously formed associations. (Formed or at least strengthened during the preliminary tests.) (3) There was practically no practice effect within any test, on account of the preliminary tests, and the use of the same material throughout. This is a condition not found in mental fatigue tests where the records are made at frequent intervals. (4) The subjects worked at their best speed, with no rest period between the first and second halves of the test. (5) There was no rest interval between the performance of separate items of the tests, such as the relaxation which may take place at the end of each addition or multiplication problem usually employed for fatigue work, especially where the time required for each problem is the unit of measure.

This falling off in performance within such a short period in the case of two subjects in the mental tests described, suggests the importance of devising simple mental tests, which shall approximate the classical ergograph test as performed upon a trained subject, in its simplicity, forced regularity of response (*e. g.*, metronome beat in the ergograph

test), and in the complete record obtained. For it is probable that mental fatigue is not so rare as is sometimes supposed, but that the repair process is so rapid compared with muscle repair, that as work is usually done, the loss may be compensated for during brief intervals of relaxation.

THE STANDARDIZATION OF KNOX'S CUBE TEST

BY RUDOLF PINTNER

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This paper deals with the results obtained with Knox's[1] Cube Test, one of the performance tests used by him for the mental classification of immigrants at Ellis Island. The test has been given to 867 normal children and a few adults, and to 463 feeble-minded individuals. An attempt has been made to enlarge the scope of the test and to standardize it a little more adequately. The test appeared to me, after first seeing it applied, to be an excellent one in many ways. Without attempting to enter into a useless discussion as to the actual mental processes involved, we may say in a general way that it depends largely upon imitation, at the same time affording every opportunity for other factors involving intelligence to assert themselves.

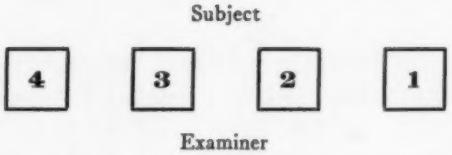
The Method of Procedure.—Five blocks are required. The Binet black cubes were used, but any other cubes of about the same size would be satisfactory, provided they are all of the same color. Four of these are placed on the table in front of the subject at a distance of about two inches apart. The examiner holds the fifth cube in his hand. He says to the subject: "Watch carefully, and then do as I do." He then taps the blocks with the fifth cube in a certain definite order and at a certain definite rate (about one tap per second), always beginning with the cube at the child's left or the examiner's right if he is facing the child. He then lays the fifth cube down in front of the child equidistant between the third and fourth cube, but nearer to the child, and says: "Do that."

These oral directions, "Watch carefully and then do as I do," and "Do that," were given to almost all of our subjects.

[1] Knox, Howard A., *The Journal of the American Medical Association*, March 7, 1914, Vol. LXII., pp. 741–747.

I do not believe this is necessary and I am sure exactly the same results would have been obtained by saying nothing. This is borne out by some of the subjects who did not understand English and by deaf children and by some children who seemed too young to understand such verbal directions. In these cases all that was necessary was to make some gesture indicating that the fifth block was to be picked up and the others to be touched.

The first line of the test, as will be noted presently, is so simple that even although the subject does not know while watching the examiner that he will be required to do the same thing, he can easily remember and imitate what has been done. There are of course innumerable combinations in which one can tap four blocks, if one is not limited to touching each cube only once. If we number the blocks the different combinations will be readily understood, and the following diagram should make absolutely clear their position with regard to the subject and the examiner (if he is facing the subject).



The following twelve combinations or lines have been used in this experiment. Number 1 always refers to the block on the left-hand side of the child.

A line..... 1 2 3 4
X " 1 2 3 4 3
Y " 1 2 3 4 2
B " 1 3 2 4
C " 1 4 3 2
D " 1 4 2 3
E " 1 3 2 4 3
F " 1 4 3 2 4
G " 1 3 1 2 4
H " 1 4 3 1 2 4
I " 1 3 2 4 1 3
J " 1 4 2 3 4 1

Knox uses only five lines. The advantages of my extension to twelve will, I think, be apparent in the light of the results. The lines are taken up in the order of sequence in which they appear above. It will be noted that this is roughly an order of increasing difficulty. A line is never repeated, not even if the subject begins any line, the *A* line included, at the wrong end. This is an error that we have marked W.E. (wrong end) and will discuss later. The subject is never corrected, but is allowed to do a line exactly as he chooses. No hint must be given the subject as to whether he is doing a line right or wrong. Some children pause and look up at the examiner waiting for a cue as to the next move. The subject is encouraged to do his best and is told that he is doing well, regardless of his actual accomplishment. Each line is marked plus or minus according as the subject does it correctly or incorrectly. If the subject corrects a wrong move, thereby making the whole line correct, he is credited with a plus.

The numbers of the moves must not be shown to the child and the examiner must not count aloud or indicate in any way that he is counting. This precaution is very necessary with intelligent older children and with adults. Nothing in fact is said about counting and the subject is left free to pursue that method if he has the intelligence to think of it. This will of course show in a greater number of lines passed correctly and therefore the subject receives credit for his intelligent adaptation. We asked every subject who did very well, *i. e.*, succeeded in almost all of the lines, how he did it. Some replied at once that they were counting. Others were uncertain. We suggested to the latter that they were counting and if they assented, we asked them and also those who told us they were counting, to count aloud while we repeated one of the longer lines or a new combination of moves. The examiner can then tell whether it is the kind of counting that will help in remembering the moves. Some children who said they remembered the moves by counting were found simply to be saying to themselves, one, two, three, four, etc., every time. This, to be sure, would help them to

remember the number of moves but would not help them to remember the position of the blocks touched. The other kind of counting which assigns constant numbers or letters to each block was found to be very rare. We found it only in about twenty cases out of our 867 individuals. This kind of counting did not seem to occur to the majority of adults who were tested by us.

In many respects this method of giving the test differs radically from that of Knox, as far as can be determined from the very brief description of the test in the article cited above. Knox allows a repetition of a line on the part of the examiner, in contrast to my procedure where a line is never repeated. For the first five of his lines he gives three trials 'if necessary,' and for his last and most difficult line involving six moves he allows five trials. If the subject fails he is evidently shown the moves over again to the extent of three or five times if necessary. The drawback of this method seems to lie in the fact that a varying number of repetitions of any line will cause unequal practice effect. For example, the subject that fails twice on the second line and passes on the third trial will have made up to that point four responses to the test, whereas the subject that passes the second line at the first trial will have made only two responses. Each will then start the third line with different degrees of familiarity with the situation, and it is possible that the first subject may have gained an unfair advantage over the second, even although the first subject is not doing as well as the second as shown by his failures. A fair comparison of their performances in the succeeding lines will nevertheless be impossible, however much the one may be superior to the other. I think that any difficulty in this respect that may exist can be adequately overcome by extending the number of lines, as has been done, by following always the same sequence and by rigidly adhering o the rule never to repeat a line.

The Subjects.—The subjects included 867 presumably normal individuals. These were in the main pupils in the ordinary grade schools of about four or five different schools

in Columbus, and some from a junior high school. Most of the five-year-olds were kindergarten children and those below that age were examined in day nurseries and settlement houses. About half of the adults tested were university students. Four hundred and sixty-three feeble-minded individuals were also tested. The vast majority of these were inmates of an institution for the feeble-minded, but there are also included in this number several feeble-minded children that were met with in the juvenile court or in school. No systematic attempt was made to exclude all cases of suspected feeble-mindedness from the data for normal children. This would have involved giving lengthy tests to some hundreds of children in the public schools. Any child who was obviously feeble-minded was excluded from the normal group and if his Binet age had been determined he was included among the feeble-minded.

The tests were not all given by the writer himself,[1] but the technic of this test is so simple that uniformity in giving it is very easily attained. I do not think that any possible error from this source would materially affect the results.

The number of normal children tested at each age is given in Table I. below, and the feeble-minded in Table IV. The normal children are grouped according to chronological age and the feeble-minded according to Binet age. It will be seen that the number for each age is not uniform, but a sufficiently large number between the ages of five and sixteen were obtained for each age. The usual difficulties were encountered in getting children below five and above sixteen years of age. The feeble-minded differed chronologically a great deal, some of them were adults and others merely children.

Tabulation of the Data.—The actual recording of the results while giving the tests is perhaps best done by making out some such blank as is shown below, which is a copy of some of the actual data.

[1] The writer wishes here to acknowledge the generous help given him in this work by Mr. Donald G. Paterson, graduate assistant in the department of psychology. He is also glad to acknowledge the assistance rendered by Miss M. Anderson and Miss A. Beekman, advanced students in the same department.

Name	Age	Grade	*A*	*X*	*Y*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*
Catherine M...	12	5B	+	−	+	−	+	+	+	−	−	+	−	−
Paul C.....	6	1A	+	+	+	−	+	−	−	−	−	−	−	−
Matilda S.	14	7A	+	+	+	+	+	+	+	+	−	−	−	−
Rosie S.	5	Kindg.	W.E.	+	+	−	−	−	−	−	−	−	−	−

During the collection of results covering in all about thirteen hundred cases the following device was employed to keep an oversight of the results as they came to hand and also to afford some clue as to when sufficient results had been collected. As the results came in they were recorded on squared paper and a curve was slowly built up. This was done for each separate line and for certain groups of lines. Fig. 1 shows the results for two lines correct out of the *BCD* lines.

The numbers along the abscissa represent the ages. A mark or dot above the line represents a correct response and a similar mark below the line represents a failure. The figure shows the building up process at the stage of completion. The figures to the left of each column represent the number of cases, the figures to the right are the percentages of correct responses for each age at different stages in the growth of the curve. When each fresh group of results was added, the percentage of correct responses was calculated and noted on the growing curve. This device for recording the results was found to add a much greater interest in the work than could have been attained by waiting until all the data had been collected, and it also shows the worker how his results are developing. If the percentages are calculated every now and then, he can see whether new results that are coming in are adding anything new to his work or are merely confirming the results that have already been obtained. If the percentage fluctuates a great deal, it is a sign that more results are required; if it remains more or less stationary for some time, it may be assumed that additional results are not likely to affect the shape of the curve. From Fig. 1 it will be seen that the percentages for most ages remained more or less constant. Some slight fluctuation is seen at age eight, where the percentage drops from 68 to 65 and then rises steadily to 74.

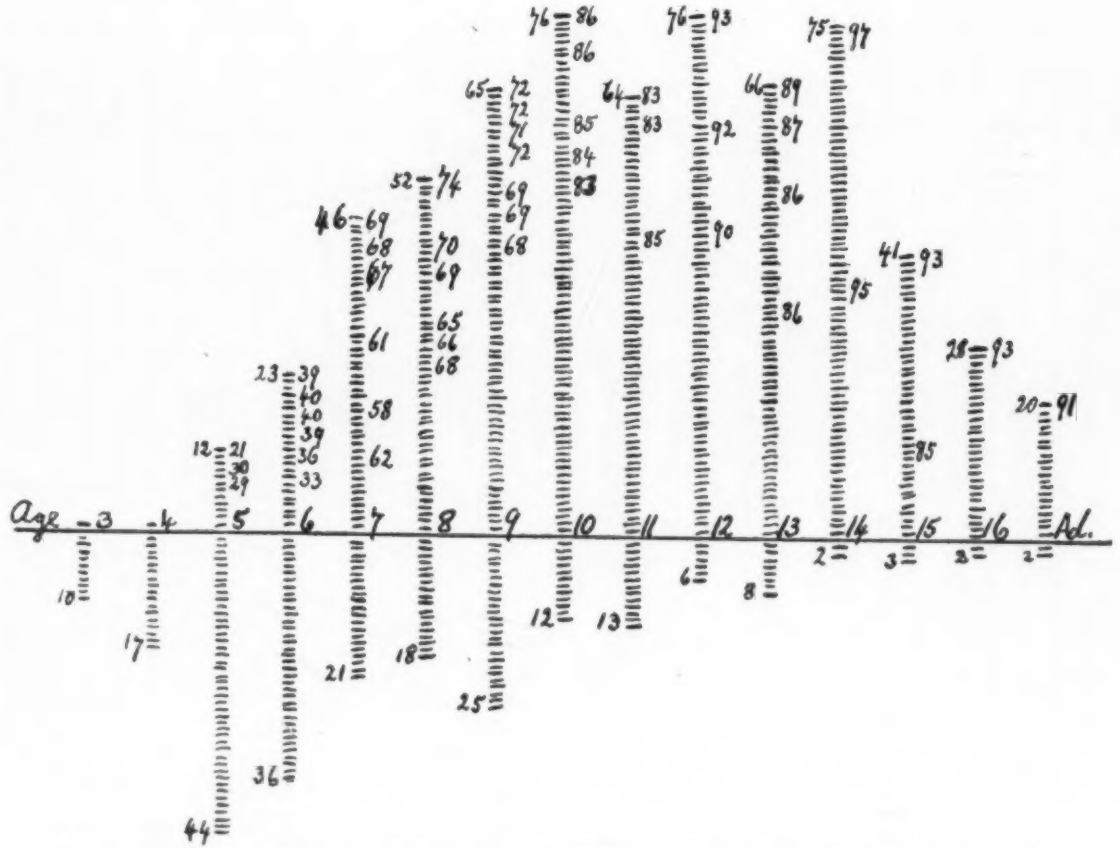


FIG. 1. Tabulation of Results for Two Correct of the *BCD* Lines. Figures to the right of the columns show percentages correct.

Still more fluctuation is seen at age seven, where the percentage at first drops from 62 to 58 and then rises slowly to 69. From the point of view of standarization this fluctuation of the percentage becomes important. We obviously cannot say that a test is a seven- or eight-year-old test, as the case may be, until we are certain that the addition of subsequent data is not likely to affect our percentages. At one stage of our curve it might have seemed that this particular combination of lines would have made a good eight-year-old test, since 68 per cent. of the eight-year-olds passed it and only 57 per cent. of the seven-year-olds. As the other results were added, however, it became obvious that a larger and larger majority of the seven-year-olds were able to accomplish it, practically as large a majority as with the eight-year-olds, and since this percentage remained more or less constant for some time, it may be safe to assume that this group of lines (to do any two correct out of the *BCD* lines) is a fair seven-year-old test.

In a similar manner curves were built up for all lines of the test and for various combinations of lines, and the comparative lack of fluctuation of the percentages seemed to show that additional data would not radically alter the results already obtained, at least with ages five to sixteen inclusive.

Standardization.—After the results had all been collected in this manner, curves for each line were drawn to show the percentage of correct responses for each age. These curves are shown in Fig. 2. The curves show how the lines compare with each other in difficulty. There are four groups of three lines each which are, as we mentioned before, about equal in difficulty. It will be seen also that most of the curves are more or less irregular and do not show any very decided increase from one age to the next, and therefore are not very satisfactory from the point of view of standardization. The actual percentages from which these curves have been drawn, together with the total number of children tested at each age, are shown in Table I.

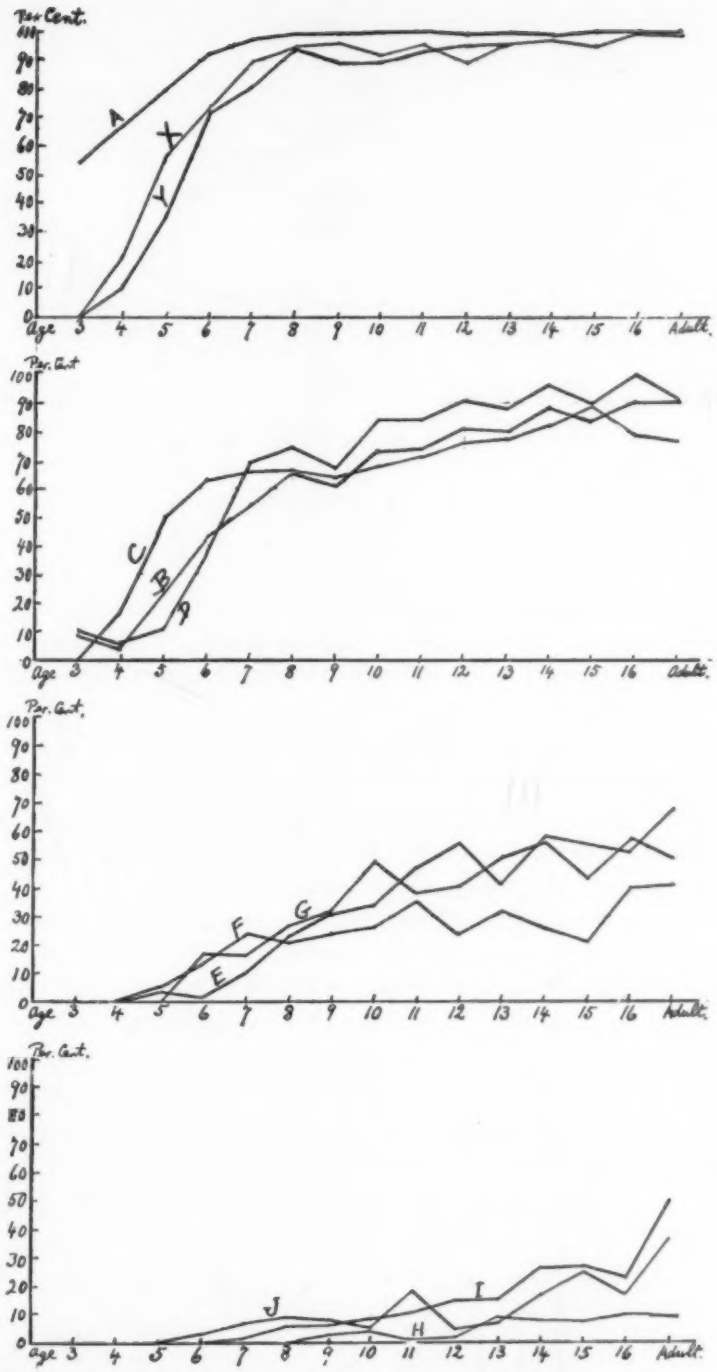


FIG. 2. Percentage of Passes at Each Age for the Twelve Different Lines of the Test.

TABLE I.

NORMAL CHILDREN. PERCENTAGE CORRECT FOR EACH LINE OF THE TEST

Chronological Age	Number Tested	Lines of the Test											
		A	*X*	*Y*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*
3	11	54	0	0	9	0	10	0	0	0	0	0	0
4	18	67	22	11	5	17	6	0	0	0	0	0	0
5	56	80	57	36	23	50	11	4	5	0	0	0	0
6	59	92	74	72	44	63	37	2	14	17	2	0	2
7	67	97	90	81	54	66	69	10	24	16	0	1	7
8	70	99	95	95	66	66	74	23	21	27	0	6	9
9	90	99	96	89	61	64	67	31	24	32	3	6	8
10	88	100	92	90	73	68	84	34	26	49	4	8	4
11	77	100	95	93	73	71	84	47	35	38	2	11	18
12	82	99	89	96	81	76	91	56	24	40	2	15	5
13	76	100	96	96	80	77	88	41	32	51	9	15	7
14	77	99	97	97	88	82	96	58	26	56	8	26	17
15	44	100	95	95	84	89	89	55	21	43	7	27	25
16	30	100	100	100	90	80	100	53	40	57	10	23	17
Adult.....	22	100	98	100	91	77	91	68	41	50	9	50	38

This brings us to the question of standardization. If we wish to include these tests in any scale of mental measurement, it is necessary to decide what line or lines of the test we should be justified in demanding that a child should pass at certain definite years in his chronological development. It has been arbitrarily assumed by some writers that a percentage of 75 or more correct responses is necessary before we are warranted in placing a test at a certain age. This seems to be taken for granted by Bobertag, Kuhlmann, Goddard and others. Stern[1] emphasises the 75 per cent. standard, without however disregarding the amount of advance at each age. Rogers and McIntyre[2] hold to the 75 per cent. basis. They say, "The standard for a pass at a given age should be determined on the basis of actual experience; our own results agree with those of Bobertag and Goddard, as about 70 or 75 per cent.," and further on, "A test was considered properly assigned to a given age when at least 70 per cent. of the children of that age were able to pass it," which shows a little weakening from the rigid 75 per cent. standard.

[1] Stern, 'The Psychological Methods of Testing Intelligence,' Trans. by Whipple. Educational Psychology Monographs, No. 13.

[2] Rogers and McIntyre, 'The Binet Simon Tests,' *The British Journal of Psychology*, Vol. VII., No. 3, October, 1914.

Bobertag,[1] discusses the matter at length and decides for the 75 per cent. standard. His discussion is undoubtedly the most exhaustive that has come within the notice of the writer. He shows the relation of this question to the normal distribution of any ability and makes a comparison with pupils' grades as given by teachers.

On the other hand Binet himself nowhere seems to have been very dogmatic on this point. As far as one can discover he seems to have considered a test standardized if passed by from 60 to 90 per cent. of the individuals tested. Similarly Terman and Childs[2] feel it impossible to adhere rigidly to the 75 per cent. standard. They say that two thirds ought to pass a given test, but they lay more stress upon "a sharp rise in ability from the year before." It may be interesting to note that the latter workers cited, Binet and Terman and Childs, have all been actively engaged in adding new tests, whereas the former are more especially those who have merely worked with the tests. It would seem to suggest that in actual practice there is some difficulty in arriving at the 75 per cent. standard.

If we now turn to actual results as shown in various standardizations of tests, we see this practical difficulty. In Goddard's[3] results we find two or three tests that have been retained in a given age where less than 75 per cent. have passed. In Bobertag's[4] results we have numerous instances where the number of passes is not up to the 75 per cent. standard, although it must be remembered that he is merely trying out the tests and not attempting to standardize them.

[1] Bobertag, 'Ueber Intelligenzprüfungen,' *Zeitschr. f. angewandte Psychologie*, Vol. 6, 1912, p. 495.

[2] Terman and Childs, 'A Tentative Revision and Extension of the Binet-Simon Measuring Scale of Intelligence,' *Journal of Educational Psychology*, Vol. 3, Nos. 2 to 5, 1912; and Terman, 'Suggestions for Revising, Extending and Supplementing the Binet Intelligence Tests,' *Journal of Psycho-Asthenics*, Vol. XVIII., No. 1, September, 1914, p. 20.

[3] Goddard, 'Two Thousand Children measured by the Binet Measuring Scale of Intelligence,' *The Pedagogical Seminary*, Vol. 18, June, 1911, p. 232.

[4] Bobertag, 'Ueber Intelligenzprüfungen,' *Zeitschr. f. angew. Psychologie*, Vol. 5, 1911, p. 105.

In Winch's[1] recent re-standardization of the Binet Scale for English children a very high percentage of passes is required before the test is admitted to a given age. But the actual numbers tested are not shown clearly and it is difficult to believe that in so many cases 100 per cent. passed the tests. Tests are shifted from one year to another rather too freely in the opinion of the present writer. If 42 per cent. pass a given test at six years, and 52 per cent. at seven, we are not warranted in assuming that the test is an eight-year-old test without testing any eight-year-old children. This seems to have been done.

From the foregoing facts and from my own experience, I am inclined to believe that it is impossible to lay down a definite percentage for the standardization of a test. It may be that theoretically about 75 per cent. should pass a given test, and probably the greater number of children we test the nearer to this theoretical standard we may attain. In actual practice, however, with large groups of unselected individuals this 75 per cent. standard is difficult to obtain, and for the practical placing of a test at a given age the crucial point seems to be the more or less sudden rise in ability from one age to another. We must require about 60 per cent. passes, but beyond that the best age for placing a test will depend upon the shape of the curve showing the percentage of passes at each age. It is significant for us to know at what age the ability of the child for a special response arises. If we were to adhere strictly to the 75 per cent. standard, we might place a test at a given age where 76 per cent. of the children pass, and where 71 per cent. of the lower age also pass. We would then be giving 71 per cent. of a lower age credit for a test of a higher age. For example, on curve *C* Fig. 2, we have 76 per cent. of the twelve-year-olds passing the *C* line, and 71 per cent. of the eleven-year-olds. It would seem to me to be obviously wrong to give the 71 per cent. eleven-year-olds credit for a twelve-year-old test standardized by this 75 per cent. procedure.

[1] Winch, 'Binet's Mental Tests; What They Are and What We Can Do with Them,' *Child Study*, Vol. VII., Nos. 1 to 8, 1914.

It will now be clear what I meant by saying above that the curves in Fig. 2 are not satisfactory from the point of view of standardization. The curves rise gradually and many of them do not show any sudden rise that might warrant us in placing the test at any special age. From my point of view we must look for some other way to make our results useful as actual tests. If we group our lines and mass the results for various combinations of lines, we get the curves shown in Figs. 3 and 4.

Curve 1 *XY* shows the percentage of those who passed one correct of the *X* or *Y* lines; curve 1 *BCD* shows those who accomplished one line correctly out of the *B*, *C* or *D* lines; curve 2 *BCD* those who did two of the *B*, *C* or *D* lines; curve 1 *EFG* those who did one of the *E*, *F* or *G* lines; curve 3 *BCD* those who did three of the *B*, *C* and *D* lines, *i. e.*, all three lines correctly; curve 2 *EFG* those who did two of the *E*, *F* or *G* lines; and curve 2 *EFGHIJ* those who did two out of the six lines from *E* to *J*. All these curves are much more satisfactory from the point of view of standardization with the exception of the curve 2 *EFG*. From this group of lines (2 *EFG*) it is impossible to get a test, at least under the age of sixteen, because the percentage nowhere rises above 50, and this is too low for the standardization of a test. Other combinations of the *E*, *F* and *G* lines were not drawn, since it is obvious that three correct out of the *EFG* lines would show still smaller percentages at all ages. Similarly the *H*, *I* and *J* lines proved themselves too difficult. The highest percentage of passes for the *H* line is 10 per cent. at sixteen; for the *I* line 27 per cent. at fifteen; and for the *J* line 25 per cent. at fifteen. Combinations of these lines would obviously not rise above 50 per cent. and so they must be discarded at least as tests suitable for ages below sixteen.

Discarding the 2 *EFG* lines, we have six curves that seem to show all the characteristics essential for the standardization of a test. Curve 1 *XY* shows a marked rise between ages four and five, from 22 per cent. to 66 per cent. and from that place onwards to 84 per cent., 95 per cent. and then re-

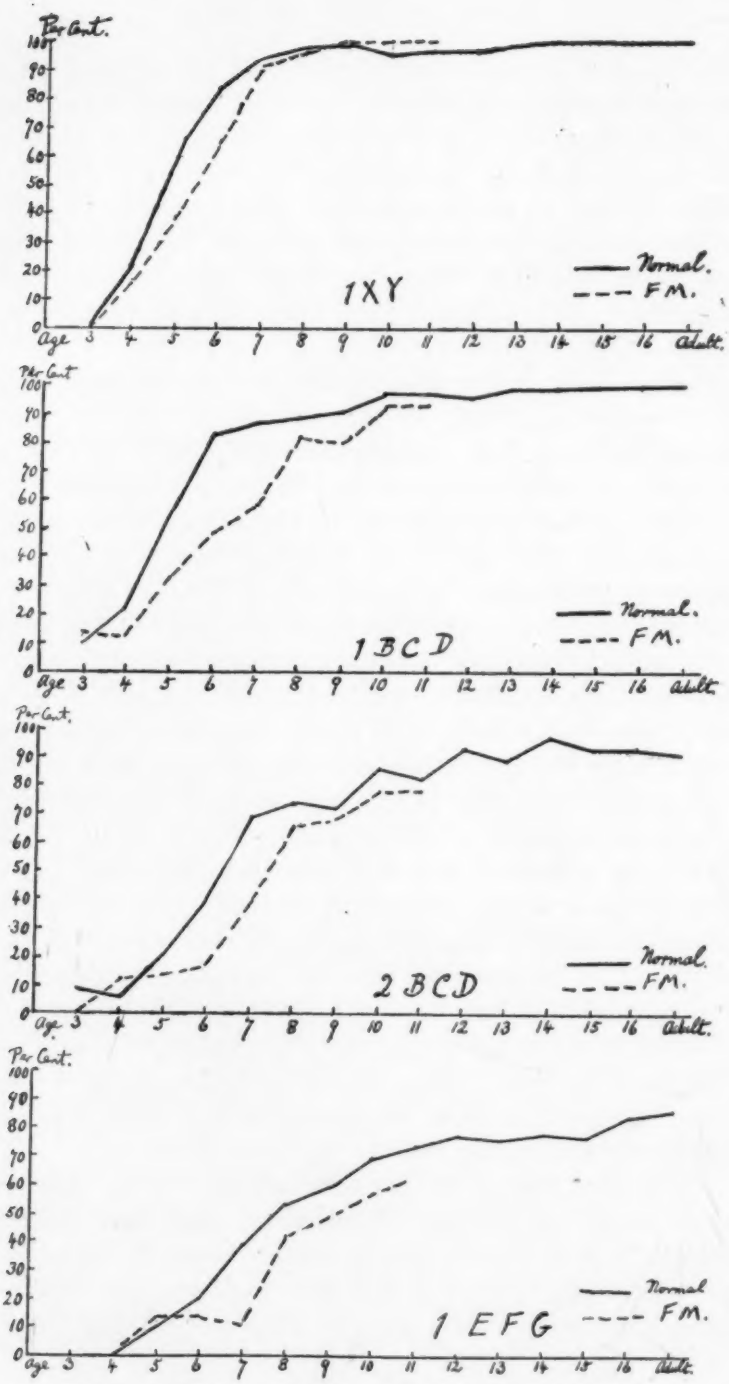


FIG. 3. Percentage of Passes for Different Combinations of the Lines.

mains in the nineties until 100 per cent. is reached at age fourteen. This means that the vast majority of children of five years and over are able to do correctly any one line of the

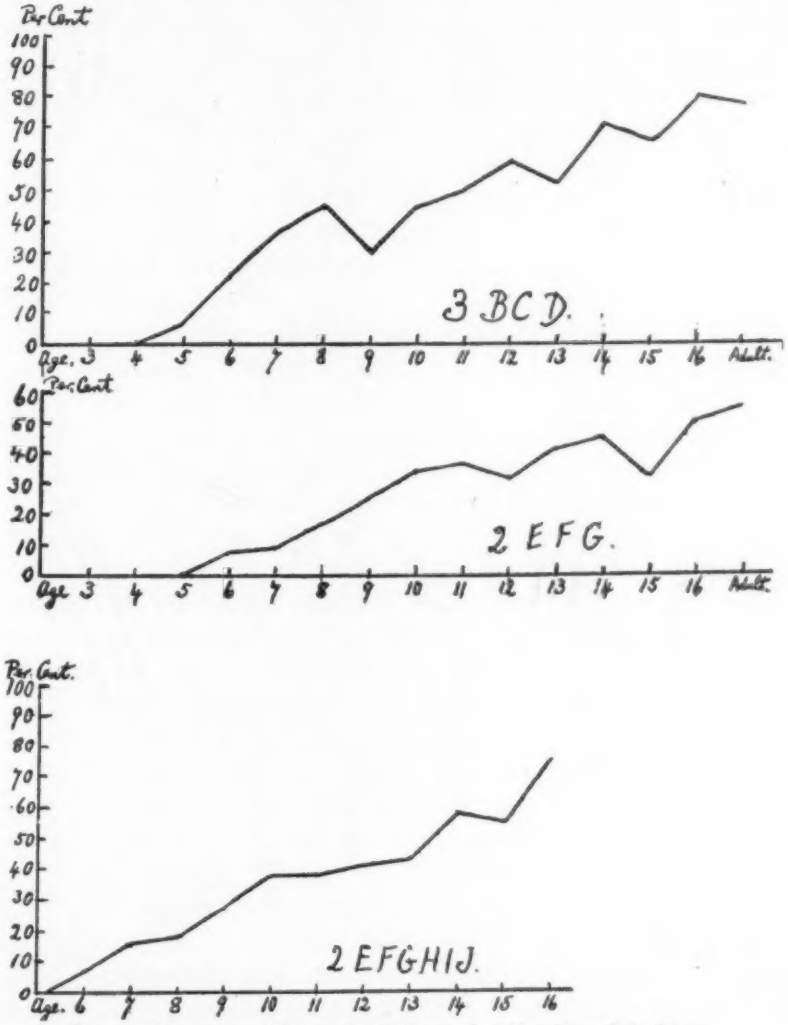


FIG. 4. Percentage of Passes for Different Combinations of the Lines.

X and *Y* lines, whereas very few of those below five years can do this. Curve 1 *BCD* shows a marked rise between five and six years, from 56 to 83 per cent. Only about half of the

five-year-olds can do one line of the *BCD* lines correctly and so it is obviously not a five-year-old test, whereas it is well within the ability of the six-year-olds. Curve 2 *BCD* shows a sudden rise between six and seven, from 39 to 69 per cent. This is obviously a seven-year-old test. Curve 1 *EFG* shows its most significant rise between nine and ten, from 58 to 69 per cent. This is not such a marked rise as in the other cases, but is probably sufficient to warrant a placing of the test at ten years. It would seem in general that the higher the ages the less marked are the rises in the curves, owing to the well-known fact that one year in the lower ages means a greater advance in the development of intelligence than in the higher ages. It would seem to me that this test is correctly placed at ten years, since the difference between the percentages at nine and ten, 58 and 69 per cent., is greater than the differences between the percentages at ten and eleven, 69 and 73 per cent., or between those at eleven and twelve, 73 and 77 per cent. This curve would also illustrate well the fallacy of adhering rigidly to the 75 per cent. standard. If we were to do this we would have to consider it a twelve-year-old test and give such credit to 73 per cent. of the eleven-year-olds and to 69 per cent. of the ten-year-olds. Curve 3 *BCD* shows a marked rise between thirteen and fourteen years, from 52 to 71 per cent. It is to be noted, however, that the twelve-year-olds with 59 per cent. passes do better than the thirteen-year-olds with only 52 per cent. passes. But the difference between 59 and 71 per cent. seems sufficient to place the test at age fourteen. Again fifteen-year-old children do more poorly on this combination than do the fourteen-year-olds, only 65 per cent. passing. This may be explained, perhaps, by the fact that the majority of these were pupils in the grades and very probably slightly below normal. The sixteen-year-olds do well. A good many of them were high school pupils.

It was surprising to me to find that a correct passing of all the three lines, *BCD*, is delayed until about the fourteenth year. Six-year-olds can accomplish one and the one passed by most of them is the *C* line. With children between the ages of six to thirteen we find a large number of them passing

each line regarded singly, (see curves *B*, *C* and *D*, Fig. 2), but it seems extremely difficult for the same child to pass all three without a mistake. Perhaps the close attention demanded is beyond the powers of the younger child. Or again the similarity of the moves may be confusing to the child, when line follows line with only a slight difference between them.

Curve 2 *EFGHIJ* shows a marked rise between fifteen and sixteen years, from 55 to 75 per cent., although we have 59 per cent. of the fourteen-year-olds passing this combination. The rise from 59 to 75 per cent. is no doubt sufficient to standardize this test and yet I feel some doubt in regard to the results in this case. The fifteen-year-old children were not on the whole as typical of their age as the children of the other ages. They were mainly children from the grades with a sprinkling of high school students. Almost every curve shows their deficiency as contrasted with the fourteen-year-olds. It may be therefore that two out of the *E* to *J* lines is a fifteen-year-old test. This is possible, although in view of the results obtained not very probable.

Table II

Normal Children. Percentage Correct for Various Combinations of the Lines of the Test

Chronological Age	Number Tested	Combinations of Lines					
		1*XV*	1*BCD*	2*BCD*	1*EFG*	3*BCD*	2*EFGHIJ*
3	11	0	10	9	0	0	0
4	18	22	22	6	0	0	0
5	56	66	56	21	11	7	0
6	59	84	83	39	20	22	8
7	67	95	87	69	39	36	16
8	70	98	89	74	53	45	18
9	90	99	91	72	58	30	28
10	88	96	97	86	69	44	38
11	77	97	97	83	73	50	38
12	82	97	96	93	77	59	41
13	76	99	99	89	76	52	43
14	77	100	99	97	78	71	59
15	44	100	100	93	77	65	55
16	30	100	100	93	84	80	75
Adult....	22	100	100	91	86	77	—

Table II. shows the results for the six combinations, which we believe can be satisfactorily used as tests. In each case

the number tested at each age and the percentage of correct responses is given. It will be seen that an attempt was made to test large numbers of children at each age. Even although the experimenter often felt that a given line was far too difficult for a child, yet it was given in order to get the negative results, without which no real standardization of a test is possible. It is just as important to know that the children below a given age cannot pass the test as to know that those of a given age can pass it, and the comparison between the two ages is not a just one unless equal numbers of both ages in question have been tested. Much of the work in standardization done up to the present time is open to just this criticism. In Goddard's figures for the revision of the Binet Scale we find tests placed at a given year because of a high percentage of passes obtained from about 100 children, whereas only between thirty to forty children and sometimes even fewer of the next lower age had been tested. From my experience with the results from this test and with others upon which work is now being done, it seems to me to be dangerous to take for granted that a few results in the age below the test age are sufficient negative evidence to exclude the possibility of the test falling at that age. It is generally conceded that the tests at the lower end of the Binet Scale are too easy and an inspection of Goddard's figures will show that in the six-year-old tests, for example, only about half and in some cases less than half of the children at the test age were tested in the age below the test age. In Table II. it will be seen that the first test of the Knox Cubes—1 *XY*—is open to just the criticism that I have been urging. There are 56 five-year-olds and only 18 four-year-olds. This is owing to the fact that I experienced great difficulty in getting four-year-olds. It may be that if fifty four-year-olds were tested, this combination of lines might prove a four-year-old test, and yet I do not believe this would be the case and so I have considered it a five-year-old test. To none of the other tests can this criticism apply. Large numbers above and below the test age have been tested and there are sufficient negative results below the test age in each case.

Total Number of All Lines Passed.—The results of the test were also tabulated in another manner to show the number of lines passed correctly at each age. The total number of lines passed correctly by each child was added together and the average and average deviation for each age found. These figures are shown in Table III.

TABLE III

AVERAGE NUMBER OF LINES PASSED AT EACH AGE

Chronological Age	Av. Number of Lines Passed	A. D.	
3	0.57	0.49	
4	1.43	0.76	
5	2.41	1.17	77% pass 2 or more lines.
6	4.22	1.25	71% pass 4 or more lines.
7	5.12	1.28	70% pass 5 or more lines.
8	5.60	1.18	
9	5.59	1.34	
10	6.29	1.32	72% pass 6 or more lines.
11	6.68	1.76	
12	6.66	1.10	
13	6.66	1.41	
14	7.55	1.24	75% pass 7 or more lines.
15	7.45	1.66	
16	8.05	1.11	62% pass 8 or more lines.

It will be seen that there is an almost steady increase in the number of lines accomplished from age four up to age eleven, where it becomes almost stationary at 6.6, to rise again at age 14 up to age 16. In no case is the average deviation larger than 1.7.

The results in this form may not be so suitable for the placing of a test at a definite age, but they could of course be incorporated in a scale. They show a very close correlation with the lines which I have considered standardized. Age four can do one line, that is the *A* line, and they are not able to do one out of *XY*. Age five can do two lines, that is the *A* line and either *X* or *Y*. Age six can do four lines, that is the *AXY* lines and one out of *BCD*. Age seven can do five lines, that in *AXY* and two out of *BCD*. Ages eight and nine cannot do more than five lines, that is *AXY* and two out of *BCD* and not one out of *EFG*. Age ten can do six lines, that is *AXY* and two out of *BCD* and one out of *EFG*.

Ages eleven, twelve and thirteen all show an accomplishment of a little more than six but not seven lines, that is they can do *AXY*, two out of *BCD* and one out of *EFG*, but they cannot do all *BCD* nor two out of *EFG*, as our curves have already shown us. Age fourteen and above can do seven lines, that is *AXYBCD* and one out of *EFG*. Age sixteen can do eight lines, that is *AXYBCD* and two out of the remainder. All this serves to corroborate our placing of the lines at particular ages. There is of course no reason why this method should not be used in crediting for different ages. In that case we should expect at five years two lines to be passed correctly; at six years four lines; at seven years five lines; at ten years six lines; at fourteen years seven lines and at sixteen years eight lines.

There does not seem to be very much difference in marking between the two methods. In 33 cases examined nineteen gave the same results for both methods; the other cases showed a slightly lower age estimate when the system of marking by total number accomplished correctly was used.

In twenty cases where the Binet ages were available for comparison the estimate of age from performance on the Knox Cubes agreed with the Binet age in seven cases by the group method and in eight cases by the total number method. The average amount of difference between the Binet age and the age as estimated by the Knox Cubes was 2.17 years for the group method and 2.36 for the total number method of using the cubes. Apart from the slight differences in results obtained by these two methods of marking for the test, it is also interesting to note how well on the whole the estimate of mental age agrees with that arrived at by the Binet Scale. It is surprising that any one test should come so near the result arrived at by a whole series of tests. I do not mean by this to suggest that the Knox Cube Test alone should ever be used to compute mental age. It must, of course, take its place in a series of mental tests.

Perseveration and Reverse Order.—Two definite types of errors occur in this test to warrant a few words. The first I have called perseveration. It consists in repeating the *A* line

after the examiner has continued with other lines of the test. The child does not seem to notice that the *X* or *B* or *C* line, as the case may be, is different from the *A* line with which he started. There is a perseveration of the tendency to tap the blocks in the order given first, which also happens to be a very easy, natural order. The child taps one, two, three, four, again and again, sometimes adding some other moves to these first four. This error of perseveration is not common among the older children. Only one of our fourteen-year-olds did this, and from his performance in other tests it is very probable that this child is defective. Of the other children we found one case at 12 years, one at 9, one at 8, five at 7, three at 6, six at 5, one at 4, and two at 3, and it is to be remembered that relatively few three- and four-year-olds were tested. It would seem then that this error is due to the lower stage of intelligence of the child, and perhaps one that may be a clue to possible feeble-mindedness. This idea is strengthened by the frequency of this error among our feeble-minded cases. This occurred as follows:

At mental age	2	3 times
	3	2 "
	4	10 "
	5	16 "
	6	22 "
	7	10 "
	8	3 "
	10	2 "
	11	1 "

This makes a total of 72 cases among the feeble-minded as contrasted with 21 among the normal children. With the feebleminded as with the normal, we notice a larger percentage of cases among the children of lower mental age.

The second error is that of reverse order, *i. e.*, beginning with the block at the child's right instead of with the block at his left, which is the one touched first by the examiner. This was noted as "wrong end." It occurs most frequently with the *A* line and rarely is peristed in for more than three or four lines. It is again an error found more often among the younger children, but it cannot be said to be particularly

characteristic of the feebleminded. The following number of cases were noted:

Normal		Feeble-minded	
Chronological Age		*Mental Age*	
Age 3	1 time.	Age 2	2 times.
4	4 times	4	2 "
5	7 "	5	1 "
6	4 "	6	2 "
8	1 "	7	1 "
		8	1 "

No credit was allowed for this error. It was treated always as a mistake, even although the line, whether the *A* line or a more complicated line, was in other respects correctly performed. In a system of mental classification giving a number of points for each test or computing by whole or half credits, it might be justifiable to given this error a half credit or a fewer number of points.

The Feeble-minded.—Four hundred and sixty-three feebleminded individuals were given the same test. These were inmates of an institution[1] and all of them had been graded by the Binet Scale. They have been classified according to mental age and the percentage correct for the different lines is shown in Table IV. and for the various combinations of lines in Table V. Their performance in some of the combinations of lines is shown by curves on Fig. 3. On the whole

TABLE IV

FEEBLE-MINDED. PERCENTAGE CORRECT FOR EACH LINE OF THE TEST

Mental Age	Number Tested	Lines of the Test											
		A	*X*	*Y*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*
2	6	50	0	0	0	0	0	0	0	0	0	0	0
3	7	57	14	0	0	0	14	0	0	0	0	0	0
4	17	76	17	8	6	12	12	0	0	0	0	0	0
5	37	92	32	21	19	27	8	5	8	13	0	0	0
6	67	97	61	38	19	40	13	7	6	3	0	0	0
7	85	99	87	54	31	41	36	4	2	8	0	0	2.5
8	73	99	98	82	55	62	71	20	26	19	1.5	1.5	4
9	75	100	97	79	45	71	67	25	24	20	2.5	5	7
10	69	100	89	92	69	67	87	35	33	30	0	10	9
11	27	100	100	100	66	70	89	41	29	29	7	15	11

[1] The writer wishes here to acknowledge the kindness and courtesy of Dr. Emerick, superintendent of the Ohio Institute for the Feeble-minded.

TABLE V

FEEBLE-MINDED. PERCENTAGE CORRECT FOR VARIOUS COMBINATIONS OF THE LINES OF THE TEST

Mental Age.	Number Tested	Combinations of Lines				
		1 *XY*	1 *BCD*	2 *BCD*	1 *EFG*	2 *EFG*
2	6	0	0	0	0	0
3	7	14	14	0	0	0
4	17	17	12	12	0	0
5	37	39	32	14	14	9
6	67	65	49	17	14	3
7	85	92	59	38	11	3.5
8	73	97	82	66	42	22
9	75	100	80	68	49	16
10	69	100	93	78	57	30
11	27	100	93	78	63	22

the curves follow those of the normal children pretty closely, generally remaining a little below. This means that the normal children as a whole do slightly better on this test than the feeble-minded of corresponding mental age. In the lower ages—three and four—the curve for the feeble-minded is very close to and sometimes even rises above the curve for the normals. This corresponds to the well-known fact that the Binet Scale is too easy at the lower end. Only in one curve, 1 *XY*, do we see the feeble-minded curve rising above the normal curve. In all the other curves it remains below the normal, although sometimes it is very near the normal. The feeble-minded individuals of the higher mental ages, ten to eleven, do not surpass normal children of corresponding chronological age, so that in this test we find no corroboration of the fact that the Binet Scale is too difficult at the upper end. This is, of course, not to be taken as a denial of the difficulty of the higher tests of the Binet Scale, although none of the tests have been subjected to the rigid standardization as has been undertaken for this test. It may be that this cube test is testing something that is not tested by the tests at the upper end of the Binet Scale. It demands concentration of attention to a continuously varying task and it requires the subject to work at a pace that is set for him. The feeble-minded are not by any means lacking in attention and perseverance. Many will work for long stretches of time at a

task with the greatest concentration of attention, but if their attention is required for a certain definite period to a changing stimulus, it seems difficult for them to adjust their attention to the continuously varying aspects of the problem. It is probable that many of the feeble-minded would do much better if the blocks were tapped not at a given constant rate, but at a rate varying with their ability to shift their attention from the one block to the other. In terms of rhythm, we might say that the feeble-minded individual is too dependent upon his own individual rhythm, and that he lacks the capacity of adjusting himself readily to external rhythms.

Summary.—The Knox Cube Test given to normal children and standardized for the different ages gives the following tests:

1 out of *XY* lines	5	year test.	
1 out of *BCD* lines	6	" "	
2 out of *BCD* lines	7	" "	
1 out of *EFG* lines	10	" "	
3 out of *BCD* lines	14	" "	(probably).
2 out of *EFGHIJ* lines	16	" "	(probably).

In actually using this method it would seem well to credit the child with the age at which the most difficult combination is passed. The child who passes 2 *BCD* is credited with a five-, six- and seven-year-old test, even although he may have failed in one of the easier combinations, the presumption being that the failure was not due to an inability to pass these easier lines, but to some disturbing factor foreign to the test.

The actual number of lines passed may also be used as an index to mental age, and if this method is followed we must give credit in this manner:

For 2 or 3 lines	5	-year credit.	
" 4 lines	6	"	"
" 5 "	7	"	"
" 6 "	10	"	"
" 7 "	14	"	"
" 8 "	16	"	"

In using this test it would be well to follow strictly the directions given at the beginning of this article, since any

deviation from this method is likely to give different results. Touch the blocks at a uniform rate, beginning with the cube at the child's left, never repeat a line and do not give the child any suggestion of counting.

In conclusion, the justification for a long article such as this one that deals solely with a single test may be found in the fact that up to now we have been more or less satisfied with a very indifferent standardization of the mental tests that are being widely used in computing mental age. We have, in fact, been avoiding the hard work and an inadequate solution of the problem of standardization. Only by a thoroughgoing treatment of each and every test, such as has been attempted here, will we ever arrive at tests that will give us something more than a mere approximation to a child's mental age. A scale made up of different tests standardized in this fashion might lay claim to some exactitude. I think emphasis must be laid on the number tested before we can rest satisfied with the standardization of any test, and in this connection it seems most important to me to have practically the same number of cases in the ages immediately above and below the test age, as we have in the test age itself. We dare not assume that the age below the test age cannot accomplish a given test unless we have sufficient negative results in that age. Our knowledge of general intelligence and the development of intelligence is so limited that it is very dangerous to take anything for granted on an a priori basis.

THE ADEQUACY OF THE LABORATORY TEST IN ADVERTISING

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That it is possible, by means of a simple experiment, to tell even roughly the relative amount of business which each of a series of advertisements will bring in, is a revolutionary idea. The feasibility of such a prediction, however, has been indicated by the writings of Strong and Hollingworth. Granting this assumption to be true, it opens up an entirely new field of experimentation for the practical psychologist. Not only that, but it should result in the development of a series of principles which would be of the greatest benefit to the advertising man. The historical summary will show the evidence upon which the assumption is based.

Historical Summary

Chronologically, the first experiment which I have found comparing the results obtained by laboratory and business methods was performed with a set of five Bullard Lathe Advertisements.[1] Ten subjects, mechanics and engineering students, were used. They were told to arrange the advertisements 'in the order in which you would buy the machine.' From the results, the relative position of each advertisement in the series was obtained. The order as determined in this way was then compared 'with the actual number of replies for catalogues received by the Bullard Co. from each advertisement.' It was found that the two orders agreed perfectly.

Another similar experiment was performed with a set of Packer's Tar Soap advertisements.[2] Fifty advertisements

[1] Strong, 'The Relative Merit of Advertisements,' 10–11. Hollingworth, 'Advertising and Selling,' 8–10.

[2] Strong, 'The Relative Merit of Advertisements,' 11–15: 63–81; *Jour. of Phil., Psy.*, etc., VIII., 600–606. Hollingworth, 'Advertising and Selling,' 11–14.

were arranged by twenty-five subjects 'in the order in which *you* would *buy* the soap.'

"When the order was compared with the order submitted by Mr. Edward A. Olds, Jr., of the Packer Manufacturing Company, and with the one from the Blackman-Ross Advertising Agency, we found a high degree of similarity between the three orders. The resemblance between the experimental order and either of the other two is equal to a coefficient of correlation of plus .52. The resemblance between the order of the Packer Manufacturing Co. and the Blackman-Ross Agency is equal to plus .64. There is then nearly as great agreement between the experimental order and that of the Packer Manufacturing Co. as between the latter and the agency, which is now handling their advertising business."

Eight advertisements out of the 50 were then selected for a more detailed study. These advertisements were arranged by 100 subjects, 60 men and 40 women. The following coefficients of correlation were found to exist:

Between 100 subjects and 25 subjects	.947
" 100 subjects and Packer Co.	.893
" 100 subjects and B.-R. Agency	.866
" 25 subjects and Packer Co.	.840
" 25 subjects and B.-R. Agency	.920
" Packer Co. and B.-R. Agency	.866

A third set of experiments was performed with Electric Light Advertisements.[1] There were originally in the set five advertisements, but only three were used. For in two of them there was a difference in one respect which affected the company's data so much as to render them unsuitable for the experiment. Thirty-six subjects were used. Working the results of the three advertisements out by the order of merit method, Strong found a coefficient of correlation of plus 1.00 between the order as determined by the laboratory test and by the business returns.

Hollingworth,[2] who gives what are apparently the returns from all five advertisements in the set, finds a coefficient of

[1] Strong, *Jour. of Ed. Psy.*, IV., 393–404.

[2] Hollingworth, 'Advertising and Selling,' 14–15.

correlation of plus .60 between the laboratory returns and the business returns as measured by the cost per inquiry.

There have been made, then, three tests, the reports of which are accessible in the psychological literature, determining the correlation between the laboratory test and the business test. The lowest coefficient is plus .52; the highest is plus 1.00. The average is in the neighborhood of plus .82. The general conclusion drawn is that the laboratory test is a satisfactory preliminary for any set of advertisements which is to be used in business. For by it the poorer advertisements can be eliminated and only the best kept. The business returns and the laboratory returns agree so closely that each can, in general, be used as a measure of the other.

Strong[2] also worked out the coefficient of correlation between the results of groups of persons coming from different walks of life. His general conclusion is: "A group of 50 college students will represent very closely the judgment of groups of educated business men and women, of young business men, such as attend evening schools, etc., and of women of the middle class regardless of age. They will not represent at all the judgment of groups from small towns and farming sections such as the regions around Garrison, N. Y., from which the data were obtained.

"It is fair to extend the results as set forth in previous chapters regarding the judgment of college students to groups of educated men and women in general. But as the data of this report are mainly concerned with cheap articles in common use, very little can be postulated concerning the relation of various groups of individuals with regard to more expensive commodities."

Experimental Results

In view of the last sentence of the quotation given above, it was thought that some profitable data might be disclosed by the study of an entirely different kind of advertising material, such as that of a mail order business.

The experimental work was done by John S. Deuble, in the

[2] Strong, 'The Relative Merit of Advertisements,' 62.

psychological laboratory at the University of Michigan. The results were put in their final form by the writer.

Three sets of advertisements were tested with a total of 161 subjects, 69 men and 92 women. The sets of advertisements were as follows: 4 half-page advertisements of the American Collection Service, Detroit, Mich., procured from Mr. William A. Shryer; 10 full-page advertisements of the American Collection Service; 9 quarter-page advertisements, *Saturday Evening Post* size, of the Burroughs Adding Machine Co., obtained from Mr. William A. Hart of the advertising department of the Burroughs Company.

The following data concerning the American Collection Service advertisements were furnished: the number of insertions, the total number of inquiries, the advertising cost, the cost per inquiry and the profit or loss for each advertisement.

From the Burroughs Adding Machine Co. the following data were received concerning each advertisement: the number of inquiries, the number of trials, the number of sales, the total amount received from the sales.

From these data, it should be possible to determine with some accuracy which of the advertisements was of the greatest value from the business standpoint. The settling of this question is, however, more complicated than appears on the surface. Any one of the points mentioned may be considered as a test of efficiency. The trouble is that they may not agree to any remarkable extent. To indicate this, the various measurements are put in the form of tables which will show the order of merit according to the different standards. In the tables throughout the paper, number 1 indicates that the advertisement so designated was the best from the standpoint of the standard used; number 2, that it was in second place, etc.

AMERICAN COLLECTION SERVICE

Half-Page Advertisements

Ad.	Average Number of Inquiries	Cost per Inquiry	Profit
A.....	1	1	1
B.....	2	3	2
C.....	3	2	4
D.....	4	4	3

Full-Page Advertisements

E.....	3	3	6
F.....	10	9	5
G.....	5	6	9
H.....	6	5	7
I.....	4	2	2
J.....	8	7	3
L.....	7	8	10
M.....	9	10	8
N.....	1	1	1
O.....	2	4	4

BURROUGHS ADDING MACHINE CO.

Ad.	Inquiries	Trials	Sales	Amount
1872 *A*.....	5	4	2	2
1792 *A*.....	3	2	3	3
1803 *A*.....	4	7	5	6
1804 *A*.....	6	6	8	9
1820 *A*.....	2	3	4	5
1832 *A*.....	8	5	7	7
1844 *A*.....	7	9	6	4
1870 *A*.....	9	8	9	8
1883 *A*.....	1	1	1	1

It is evident from these tables that the measurement of efficiency which is used is of some importance. The coefficient of correlation[1] between the orders as established by the various possible measurements will show their resemblance.

AMERICAN COLLECTION SERVICE

Correlation between

	Half Page	Full Page
Inquiries and cost per inquiry.....	.80	.915
Inquiries and profit.....	.80	.430
Cost per inquiry and profit.....	.40	.624

BURROUGHS ADDING MACHINE CO.

Correlation between

	Quarter Page
Inquiries and trials.....	.790
Inquiries and sales.....	.834
Inquiries and amount.....	.650
Trials and sales.....	.773
Trials and amount.....	.617
Sales and amount.....	.933

These figures indicate that the busines test which is used has a considerable influence on the coefficient of correlation

[1] The coefficients of correlation are worked out by the formula given by Myers, 'Text Book of Experimental Psychology,' 1909, page 131. The formula is $r = 1 - [6 \text{ sigma } (d)^2/n(n^2 - 1)]$.

between it and the laboratory test. For reasons which will be explained more fully in a later part of the paper, it appears that the average number of inquiries per insertion of the advertisment is the fairest measure of the pulling power of the advertisement under actual business conditions. This measure was taken because it seems to be the fairest test of the actual pulling power, undisturbed by such things as a follow-up system, the arguments of salesmen, etc.

The experiment was carried on as follows. The subject was handed a series of advertisements and told to look them over carefully. Having become familiar with them, he was instructed to pick out the one which was to him the most persuasive. By persuasive was meant the one which would be most likely to make him answer the advertisement. This done, he was asked to pick out the second best, the third, and so on, until he had the series arranged in a descending order from the most persuasive to the least persuasive. A first choice was given a credit of 1, the next best a credit of 2 and so on down throughout the entire series. When the 161 subjects had arranged the set of advertisements, the credits which each advertisement had received were added and divided by 161, thus giving the average place which the advertisement occupied in the opinion of the 161 subjects. The one which received the smallest average was considered to have the greatest pulling power; the one which obtained the largest average was credited with the least pulling power.

In the tables below are given the averages for each advertisement as worked out in this way, together with the average deviation (A. D.). The reactions of the men and of the women are not given separately, for no significant differences were found.

AMERICAN COLLECTION SERVICE

Half Page

Ad.	Average	A. D.	Position
A.....	1.84	0.74	1
B.....	1.93	0.84	2
C.....	2.81	0.81	3
D.....	3.31	0.75	4

Full Page

E	6.13	2.31	7
F	4.47	2.69	3
G	6.00	2.00	6
H	4.83	2.19	4
I	6.78	2.14	9
J	5.38	2.13	5
L	6.60	1.97	8
M	4.12	2.29	2
N	7.71	1.80	10
O	2.98	1.67	1

BURROUGHS ADDING MACHINE CO.

Ad.	Average	A. D.	Position
1	3.95	1.93	2
2	6.87	1.88	9
3	4.54	2.34	5
4	4.52	2.02	4
5	6.70	1.92	8
6	4.00	1.89	3
7	5.32	2.42	6
8	6.37	1.77	7
9	3.71	1.72	1

The tables show that a more or less definite order of the relative persuasiveness of the advertisements in these three sets has been worked out. The average deviations indicate that some changes in order might occur if more subjects were tested, but the extremes at least are fairly well defined.

As a check upon the probability of the final order of the series as determined by the experiment, the following method was used. The order was determined by averaging the results of the first 10 subjects, then of the first 20 and these orders compared. Then the order of the first 20 and the first 30 was compared, and so on for the 161 subjects. If the final order was established relatively early in the series and persisted without actual change throughout, it was thought that the final order had a high degree of probability. The question of the number of subjects necessary in an experiment of this sort is always an important one and one which it is difficult to settle off-hand. Such a test as the one described is an entirely practical measurement which can be applied at any time in the experiment.

It was found that the final order of the half-page advertisements of the American Collection Service was determined

with 30 subjects. The addition of 130 more did not change the relative order of merit, though, obviously, there were changes in the degree of merit. Advertisements *A* and *B*, even at the end of the 160 trials, might have been changed by the addition of the results of 10 more subjects if they had all given *A* fourth place and *B* first place. In the entire experiment, however, *A* was put in fourth place only 10 times, so the chances of its appearing in last place are 1 to 16. In the same way, the chances of *B* appearing in first place are 1 to 2.4. We are justified in concluding, then, that the final order of this series is determined beyond reasonable doubt for the class of subjects used.

With the full-page advertisements of the American Collection Service, the final order was determined by the 120th trial. The addition of the next 40 subjects did not affect the relative order. The probable accuracy of this series, while not so great as that of the half-page series, is sufficient for all practical purposes. Some of the advertisements in the middle of the series might have shifted one place by the addition of 10 more subjects, but this is unlikely, for in order to do so, the ten would have to give averages which were not even approached in the course of the experiment. So we may conclude that we have obtained here an order which would be very closely approximated by any laboratory test conducted upon average undergraduates.

With the Burroughs Adding Machine advertisements, it may be said a satisfactory final order never was obtained. An order was established with the 120th trial which lasted through the 150th, but the difference between the credits for two of the advertisements was so slight that they were inverted with the addition of the next ten subjects. Two other advertisements in the series might well have been inverted by the addition of another 10 subjects. So we may say that an entirely satisfactory final order never was determined for this set of advertisements. The extremes are clearly defined, but the intermediate members of the series are somewhat variable. It seems probable, in view of these facts, that this series of advertisements was of a more even, homogeneous

sort than was either of the series of the American Collection Service. This does not necessarily mean that the Burroughs Adding Machine advertisements are either better or worse than the American Collection Service series. It simply means that it was more difficult to make adequate judgments between them.

This general condition emphasizes one fact which must be plainly apparent to any student of the psychology of advertising,—namely, that our laboratory tests have been unable to tell us whether an advertisement is absolutely a good advertisement or not. The only thing such a test can do is to place it relatively in a series. The limits of goodness and badness are to be found inside of that particular series. Because one advertisement is the first of one series and another the last in a second is no reason for asserting that the former is absolutely a better advertisement than the latter.

AMERICAN COLLECTION SERVICE.
Half Page.

Ad.	Order as Determined by			
	Lab. Test	Inquiries	Cost per Inq.	Profit
A	1	1	1	1
B	2	2	3	2
C	3	3	2	4
D	4	4	4	3

The following coefficients of correlation are found to exist:

Between the laboratory test and the number of inquiries 1.00
" " " " " the cost per inquiry ..... .80
" " " " " the profit ..... .80

AMERICAN COLLECTION SERVICE
Full Page

Ad.	Order as Determined by			
	Lab. Test	Inquiries	Cost per Inq.	Profit
E	7	3	3	6
F	3	10	9	5
G	6	5	6	9
H	4	6	5	7
I	9	4	2	2
J	5	8	7	3
L	8	7	8	10
M	2	9	10	8
N	10	1	1	1
O	1	2	4	4

The following coefficients of correlation are found to exist:

Between the laboratory test and the number of inquiries.....—0.43
" " " " " the cost per inquiry.....—0.58
" " " " " the profit.....—0.01

BURROUGHS ADDING MACHINE CO.

Ad.	Order as Determined by		
	Lab. Test	Inquiries	Amount Received
1	2	3	3
2	9	4	6
3	5	5	2
4	4	6	9
5	8	2	5
6	3	7	4
7	6	1	1
8	7	8	7
9	1	9	8

The following coefficients of correlation were found to exist:

Between the laboratory and test the number of inquiries.....—0.43
" " " " " the amount received.....—0.06

The second step in the experiment is to compare the order as determined by the laboratory experiment with the order as determined by the different measures of business efficiency. This comparison is given in the preceding tables.

Taking these three sets of results, we find the following correlations between the order as determined by the laboratory test and the order as determined by the average number of inquiries.

Half-page advertisements, A. C. S.....= 1.00
Full-page advertisements, A. C. S.....=—0.43
One-fourth-page advertisements, B. A. M.....=—0.43
Average.....= 0.047

We also find the following correlations between the order of merit as determined by the laboratory test and the order determined by the profit or the amount received.

Half-page advertisements, A. C. S.....= 0.80
Full-page advertisements, A. C. S.....=—0.01
One-fourth-page advertisements, B. A. M.....= —0.06
Average.....=+0.243

These figures show simply chance resemblance between the results of the laboratory test and the average number of inquiries per insertion and very little better than chance resemblance between the laboratory test and the business test where profits are used as the measure. The indication is that the order of merit method has at least no universal application to advertising problems. Of course, not enough series of advertisements have been tested to settle the question definitely. But since two of the three tests made have shown a significant negative correlation, a fairly large number of tests which result in equally strong positive correlations will be necessary to offset the negative results of these experiments.

It is possible, too, that college students are not satisfactory subjects for the kinds of advertisements which were used. Since a fairly large percentage of the men go into business upon leaving college, they are possibly more satisfactory than would appear at first.

As will be seen below, advertisement *N* of the American Collection Service series present some queer anomalies. Even if this advertisement is left entirely out of consideration, the coefficient of correlation between the laboratory test and the business test as measured by the average number of inquiries is -0.12.

The general conclusion seems to be that the mail order business appeals to a special and limited class of individuals. College students, on the average, are not fair representatives of such a class. With a mail-order business, it is possible to get returns which are extremely accurate, so such advertisements would make the best material for laboratory tests, if such tests would only work. On the other hand, it does seem probable that the order of merit method might be applied to those commodities which are in more general demand, such as soaps, foods, and the like. It is, however, impossible to get accurate returns as to the exact amount of business which each advertisement has brought in. This renders it impossible to be sure that the laboratory test actually has a high coefficient of correlation with the business returns.

There are several interesting things which crop out in connection with the advertisements of the American Collection Service. It will be recalled that Mr. Shryer furnished two sets of advertisements, one consisting of four half-page, the other of ten full-page, advertisements. Half-page advertisement *D* was, as far as possible, an exact copy of full-page advertisement *N*. In *N* there were five pictures in connection with testimonials, and a return coupon. In the half-page advertisement *D* there were but three pictures accompanying testimonials, and no return coupon. The wording of the argument in the two advertisements was as nearly identical as possible considering the change in size. In fact, only 6 words were changed. The half-page advertisement, however, was printed in smaller type, as necessarily must be the case, considering the difference in size.

In spite of the great similarity of the two advertisements, the results of the business returns, taking the average number of inquiries as the basis, show that the full-page advertisement *N* was the best of the set of ten. The half page advertisement *D* was the poorest one of its set. With such discrepancies as this in the business returns, it would be truly remarkable if the laboratory test did show a high coefficient of correlation with the business test. The laboratory test is at least fairly consistent, for it ranked each of the advertisements, *D* and *N*, as last of the set to which it belonged.

In the business test, the presence of the return coupon in one advertisement and its absence in the other may have been a determining factor. Shryer[1] ran two half-page advertisements at different times which were just alike except that one had a return coupon, the other did not. The one with the return coupon brought 83 replies, the other 41, showing that in this particular case, the use of the return coupon more than doubled the number of inquiries. If *N* had brought only half as many replies, it would have ranked sixth in the series, while if *D* had brought in double the number of replies, it would have ranked second. However, if there is so great an increase in efficiency coming from the return

[1] W. A. Shryer, *System*, December, 1913, p. 579.

coupon, the laboratory returns, if they are adequate, should have indicated it.

It so happened that there was a great similarity between two other members of the two sets. Half-page advertisement *B* was very much like full-page advertisement *O*. The wording was practically the same, though different pictures were used. On the basis of the average number of inquiries per insertion, *B* was in second place. According to the laboratory test, it was second in the set of half-page advertisements. From the standpoint of business returns, full-page advertisement *O* was in second place; in the laboratory test, it was first. This shows a fair consistency for both the laboratory test and the business test.

The similarity between the returns from *B* and *O* is an interesting check upon the dissimilarity found to exist between *D* and *N*. For it might have been thought that the half-page advertisements were much superior in general make-up and appeal to the full-page advertisements. Logically, *D* might easily have been the worst of the half-pages, while its duplicate *N* might equally well have been the best of the full-page displays. This, however, is rendered extremely doubtful by the results obtained from *B* and *O*. The indication is that the peculiar results obtained with *D* and *N* are due to extraneous conditions which could not possibly be controlled in the laboratory.

Because of the results of this experiment as well as on account of theoretical considerations, the writer has been led to question the application of the order of merit method to advertising problems. He does not question the true usefulness of the method, but does deplore the uses to which it has been put. In his opinion, the experiments which have been performed by this method on advertising problems can be attacked on several sides.

In the first place, the experimenters have in but a very few instances compared the laboratory results with the business results. The idea of this comparison is, of course, to show the dependability of the method as a laboratory technique for investigating advertising problems. One thing which has been

done is to compare the order as determined by the laboratory experiment with the order as determined by the opinion of certain selected advertising experts, who were practically put through the same experiment. Since Dr. E. K. Strong, Jr., was one of the first to apply this method to the psychology of advertising, a quotation from one of his articles will be appropriate in bringing out the point. "It is scarcely necessary to repeat that the results of the Packer Manufacturing Company are not based upon carefully compiled data, but only upon the judgment of the firm based on their business experience. Any one familiar with advertising knows that such data have not been compiled for any extensive set of advertisements, let alone a series of fifty extending over twenty years of service. If such data did exist, it could not be used at its full face value, as an advertisement of twenty years ago might have been very effective then and be out of date to-day.

"The order of the twenty-five subjects correlates plus .52 with the order of either of the two advertising experts. The correlations between the orders of the two advertising experts is plus .64. These relationships are lower than those which have been obtained with other sets of advertisements. . . .

"It is evident, then, that the 'order of merit method' does give results that correlate high with results obtained in business."[1]

Since by results obtained in business, Strong must evidently mean, in the above connection, the opinion of advertising experts, another quotation taken from the same writer, but in a different article, will be especially interesting. "At the present time there is no way of estimating which are the good and which are the poor advertisements except on the basis of personal judgment; and when the reviews and criticisms of different advertising men are compared, it is apparent that this personal judgment is today a very variable factor."[2]

The second quotation robs the first of whatever force it might originally have had.

[1] Strong, *Jour. of Phil., Psy., etc.*, VIII., 603, 604.

[2] Strong, *Jour. Ed. Psy.*, IV., 393.

In order to be of any particular value, the correlation between the business test and the laboratory test must be worked out with actual business returns. These are obtainable for but few kinds of commodity, since they depend upon elaborate systems of keying. In order to have the keying satisfactory, all orders must eventually come to a head office, labeled in such a way that each advertisement may receive full credit for its work. Such a thing is an obvious impossibility with such products as soaps, foods, and in general those things which are procurable at stores.

The advertisements which can be accurately keyed are ordinarily mail order propositions. With any adequate system of checking returns, it is possible to figure out from keyed advertisements the following things: the average number of inquiries per insertion, the average cost per inquiry, the total number of sales, the profit or loss. Some of these returns obviously depend upon other things than the advertisement itself, but it was the advertisement which started the whole process going.

Which of these is the fairest measure of the pulling power of the advertisement? The number of inquiries indicates the number of persons who were influenced sufficiently by the appeal to be incited to action. The weakness of this method is that the position of the advertisement on the page[1] or the position of the page[2] in the advertising section of the magazine may be detrimental. The same advertisement in some other position might have pulled many more inquiries. Again, the time of year is a very important matter. There are good seasons and bad seasons.[3] General economic conditions, national or sectional eras of prosperity are also modifying factors.

[1] Hollingworth, 'Advertising and Selling,' 80–90.

[2] Starch, 'Advertising,' 106–116.

[3] Shryer, 'Analytical Advertising,' 167–170. Shryer says, on p. 169: "As a whole, however, it may be said that the three *largest* months of practically every year are January, February and March."

See also Starch, 'Advertising,' p. 50. The table at the bottom of the page shows that, for the commodity mentioned, more advertising was run and more sales were made during the first half of the year than during the last half. Another table, given by Starch on p. 93, indicates that the most advertising is carried in May and December; the least in January and August.

It seems obvious that the natural procedure in such cases would be to repeat the advertisement enough times in different parts of the magazine and at different times selected to take account of seasonal differences and so on. The objection is that with successive appearances of the advertisement there is a fairly constant and regular decrease in the number of inquiries.[1] However, if enough advertisements were used in this way, either the total or the average number of inquiries would be a sufficiently satisfactory measure of the pulling power of the advertisement. It is, in fact, the only obtainable measure of the pulling power uncomplicated by other factors.

A second possibility is the average cost per inquiry. This method is open to all of the objections noted above, and to a still further one. The actual cost of the space occupied by the advertisement does not in any way directly effect the excellence of the advertisement itself. Even in the same medium, the charge per page is liable to sudden shifts. It is unfair to the advertisement to make it suffer the handicap of the increased rate. The amount charged per page is not an accurate measurement of the circulation of the medium and so an approximation of the number of persons who may read the advertisement.

The number of sales is obviously unfair, for we have to do there not only with the advertisement itself, but with the goodness or badness of the follow-up system, the efficiency of salesmen, etc. Some of the blame may be laid to the advertisement for it may have been constructed in such a way as to have interested many who could not possibly have bought that line of goods. Or they have been misled by the advertisement and when they found out what the product was from the follow-up system, they lost interest.

The question of profit or loss resulting from the use of a certain advertisement, while of considerable interest to the business man, is still not a test of the pulling power of the advertisement, but is a measure of the pulling power as modi-

[1] Shryer, 'Analytical Advertising,' 81 ff., 220–223. Starch, 'Advertising,' 170–179. Hollingworth, 'Advertising and Selling,' 235. Strong, PSY. REV., XXI., 147.

fied by the cost of the advertisement and the adequacy of the follow-up system.

Taking it all in all, the average number of inquiries per insertion seems to be the fairest test of the actual pulling power of the advertisement. It is, then, the measurement which should be used in endeavoring to obtain the correlation between the orders of the business test and the laboratory test.

Another criticism of the order of merit method as it has often been used is on the ground of the number of subjects employed or the number of tests made. Obviously, if relatively few additional tests will change the order of the advertisements in the series, the experiment is unfinished.

From the experiments discussed above, it appears that the number of tests necessary depends upon at least two factors. In the first place, the actual amount of difference in terms of judgment steps between the contiguous advertisements in the series is an important consideration. With advertisements far apart, where the judgment is easy to make, the order will be established with relatively few subjects. But, as the judgments become more and more difficult, an increasing number of tests will be necessary. Secondly, the number of advertisements in the series will be a determining factor. For as we increase the number of advertisements in the series, we ordinarily must necessarily decrease the judgment steps, thus rendering a satisfactory arrangement more difficult.

Shryer[1] who was the first to use any considerable number of persons in an advertising experiment, employed a total of 508 in his efforts to reach practical certainty. In the most complex of his experiments, in which the method of paired comparisons was used, the final order was obtained at the 300th trial. The addition of 200 more subjects left the relative order of the advertisements in the series unchanged. In this experiment he used but five different advertisements. Had he used more than five, ten for example, he probably would have had to employ a great many more individuals

[1] Shryer, *System*, XXV., 146.

before obtaining a satisfactory final order. To be sure, his material was such that there was a great chance for variability of response, but this is true of practically all experiments carried on in the field of advertising.

Since Shryer, in his experiment, used the method of paired comparisons, his results are not strictly applicable here. The experiments which have been described in this paper indicate something about the number of tests necessary. With four advertisements in the series, the final order was determined with 30 trials; with ten advertisements, the final order was determined with 120 trials. With three other sets of ten advertisements each, which were used to test a different point, a satisfactory final order was not obtained with 100 trials. Until enough data have been obtained to work out a satisfactory mathematical law, it seems that one of the tests of an adequate number of subjects is the purely practical one which was given in the first part of this paper. Enough has been said, however, to indicate that in the majority of tests, an insufficient number of subjects has been used.

The next point to be considered is whether the order of merit method can be used to determine the relative pulling power of a series of advertisements. Before considering this point theoretically, we may repeat that the experiments which have been designed and carried on to test the correlation between the laboratory and the business test have sometimes shown correlations as high as plus 1.00 and sometimes as low as − 0.60. It would seem, then, that sometimes the method will work and sometimes it will not.

The instructions usually given in the experiment are, "Sort these advertisements according to the order in which *you* would *buy* the . . . ." That means that every individual who performs the experiment makes a definite arrangement of the advertisements, the order showing the persuasiveness as far as he is concerned. The assumption is that from his arrangement of the advertisements, it is possible to tell which one made him buy the article, for each one experimented upon is evidently regarded as a purchaser. There is, unfortunately, no way of telling which of the persons experimented

upon would, in actual life, be sufficiently interested in any of the advertisements in the series to make him purchase the commodity.

In business, the situation is quite different. The following figures, taken from Shryer,[1] will point out what is likely to happen in a mail-order business. "Let us assume a circulation of 100,000 at $100 a page—an honest rate. Let us use a page of the strongest copy, yielding inquiries at 10 cents. Let us assume a selling average of 20 per cent., just double the ordinary. We therefore secure 1,000 inquiries. We therefore sell 200 of the 100,000 or one-fifth of 1 per cent. . . . These figures are assumed figures, but they represent the outside limits of actual average results." These figures indicate that on the average the inquiries are 1 per cent. of the circulation of the magazine. It has been estimated that five persons read each magazine. There is, then, a possibility that the advertisement will be seen by 500,000 persons. The estimate of the number of persons who see the advertisements varies from 10 per cent. to 50 per cent. of the readers of the magazine. If we take the lower limit, 10 per cent., that means that 50,000 will see some of the advertisements. The proportion which will see a particular advertisement is pure guess work. As a working basis, we will take 20 per cent. That means that 10,000 will see the advertisement, and a thousand will be sufficiently interested in it to reply, or 10 per cent. A great many of the other 90 per cent. who do not inquire are almost interested enough to do so, still more are slightly interested, others are indifferent, while still others get a negative reaction. Therefore, the results obtained from the mail-order business test are got from a very small percentage of the total number of readers. The results obtained from the laboratory test are arrived at by using the results of the whole 100 per cent. of readers, instead of the 10 per cent. who would on the average be interested enough to answer the advertisement. The using of the other 90 per cent. of the persons introduces factors into the experiment which would quite certainly modify the results so that they

[1] Shryer, 'Advertising and Selling,' XXII., 24.

would not adequately express the normal results for the 10 per cent. If we only had some way of determining, in our laboratory experiments, the individuals who make up the 10 per cent. who are sufficiently interested, we probably could arrive at fairly dependable results.

It must be kept in mind that a mail-order business appeals to a very small number of persons at best. The same general situation exists, also, with regard to the more expensive commodities, such as pianos and vacuum cleaners. Such advertisements certainly appeal to a very small and select class. Consequently, it is doubtful if adequate experiments could be performed upon advertisements of these commodities in the laboratory. The cheaper, more frequently used goods, such as foods, soaps, etc., very probably could be tested adequately if there were any way of determining accurately the actual business returns.

Lastly, it is extremely doubtful if the great majority of individuals can tell which of a series of advertisements would be most likely to make them buy the advertised product. It is very much like asking a man what he would do if his house burned up in the night. The measurement of impressions in relative terms offers considerably less difficulty, as has been demonstrated in the experimental work upon sensation, esthetic judgments, and so on. Predicting probable conduct is a much more hazardous matter. It is extremely improbable that we can really tell what we will do under a hypothetical condition unless we have developed a very definite habit for meeting that situation. Then the chances are that we will have two or more habits which are about equally serviceable. Unfortunately for the advertiser, a considerable percentage of the readers of advertisements have formed the habit of appreciating advertisements and seldom if ever responding.

The reading of advertisements has become a fixed habit with many persons, not because they expect to buy anything, but because the advertisements are an essential part of the enjoyable features of the magazine. They are looked at for esthetic appreciation, they are looked at for news value, for

they give information concerning the industrial activities of the country which could never be found in the body of the magazine.

The general conclusion which we seem forced to accept is that the order of merit test is not a very adequate laboratory method for testing the business value of advertisements. Where it is possible to obtain accurate business measurements, the laboratory test, using students as subjects, appears to be quite inadequate. Where it is impossible to secure accurate business measurements, the laboratory test may be adequate. There is no way of telling.

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